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P R E S E N T S T A T E

O F

E L E C T R I C I T Y,

W I T H

O R I G I N A L E X P E R I M E N T S,

By JOSEPH PRIESTLEY, LL.D. F.R.S.

T H E T H I R D E D I T I O N,

C O R R E C T E D A N D E N L A R G E D.

*Causa latet, vis est notissima.*

*Ovid.*

V O L. II.

L O N D O N,

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C O N T E N T S.

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T H E  
H I S T O R Y  
AND PRESENT STATE OF  
ELECTRICITY.  
P A R T II.  
A  
SERIES OF PROPOSITIONS,  
COMPRISING ALL THE  
GENERAL PROPERTIES OF ELECTRICITY.

**A**FTER tracing, at large, the progress of all the discoveries relating to electricity, and giving an historical account of them, in the order in which they were made; it will, probably, be no disagreeable repetition, if I give, at the close of it, a SERIES OF PROPOSITIONS, comprising all the general properties of electricity, drawn up in as succinct a manner as possible. And, notwithstanding the large detail which has been made, it will be found, that a few propositions are sufficient to comprise almost all that we know of the subject.

THIS circumstance may be regarded as a demonstration of the real progress that has been

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made

made in this science. And as this progress advances, and the history enlarges, paradoxical as the assertion may seem, this part may be expected to contract itself in the same proportion. For the more we know of any science, the greater number of particular propositions are we able to resolve into general ones; and, consequently, within narrower bounds shall we be able to reduce its principles.

I MIGHT have made this part of my work much shorter, even in the present state of the science, if I would have admitted into it any thing theoretical; but I have carefully avoided the principles of any theory, even the most probable, and the nearest to being perfectly ascertained, in this series of propositions; in which I propose to comprehend only *known facts*; that my younger readers may carefully distinguish between *fact* and *theory*; things which are too often confounded.

I HAVE not, in this part of my work, descended to any *minutiæ*, in the description of electrical appearances, because they have been entered into before, and a repetition of them would have been tedious. At the same time, I think it will be found, upon examination, that I have not omitted to take notice of any discovery of importance. I have also introduced into it the definition of all the most necessary *technical terms*; that this part of the work might serve as a methodical introduction, to those who are beginning the study of electricity, and desire a general knowledge of  
the

the first elements of the science, before they enter upon the detail of particulars, which will be best learned afterwards, from the history \*.

By *electricity*, in the following propositions, I would be understood to mean, only those *effects* which will be called electrical; or else the *unknown cause* of those effects, using the term, as we use the letters  $x$  and  $y$  in Algebra.

ALL known substances are distributed by electricians into two sorts. Those of one sort are termed *electrics*, or *non-conductors*; and those of the other *non-electrics*, or *conductors of electricity*.

METALS of all kinds, together with semi-metals, and water are conductors. So also is charcoal, and other substances of a similar nature, as will be shewn at large in the last part of this work. All other substances, whether mineral, vegetable, or animal, are non-conductors. But many of these when they are made very hot, as glass, rosin, baked wood, and, perhaps, all the rest on which the experiment can be made in this state, are conductors of electricity.

ALL bodies, however, though in the same state of heat and cold, are not equally perfect electrics, or perfect conductors. Vegetable

\* Lest this part of the work should not prove a sufficient introduction to the study of *electricity*, I have since published a small piece with this title. It contains a more familiar explication of the fundamental principles of electricity, mixing theory with facts, and illustrating, chiefly, those experiments which are the most entertaining.

and animal substances, for instance, in their natural state, are seldom perfect electrics, on account of the moisture that is contained in them. And, independent of moisture, there is probably a gradation in all substances, from the most perfect conductors to the most perfect non-conductors of electricity.

It is the property of all kinds of electrics, that when they are rubbed by bodies differing from themselves (in roughness or smoothness chiefly) to attract light bodies of all kinds which are presented to them; to exhibit an appearance of light (which is very visible in the dark) attended with a snapping noise, upon the approach of any conductor; and, if the nostrils be presented, they are affected with a smell like that of phosphorus.

An electric substance exhibiting these appearances, is said to be *excited*, and some of them, particularly the tourmalin, are excited by heating and cooling, as well as by rubbing.

It is necessary, however, to a considerable excitation of any electric, that the substance against which it is rubbed (usually called the *rubber*) have a communication with the earth, or bodies abounding with electricity, by means of conductors; for if the rubber be *insulated*, that is, if it be cut off from all communication with the earth by means of electrics, the friction has but little effect.

When insulated bodies have been attracted by, and brought into contact with any excited electric, they begin to be repelled by it,  
and

and also to repel one another: nor will they be attracted again, till they have been in contact with some conductor communicating with the earth; but, after this, they will be attracted as at first.

IF conductors be insulated, electric powers may be *communicated* to them by the approach of excited electrics. They will then attract light bodies, and give sparks, attended with a snapping noise, like the electrics themselves. But there is this difference between excited and communicated electricity, that a conductor to which electricity has been communicated parts with its whole power at once, on the contact of a conductor communicating with the earth; whereas an excited electric, in the same circumstances, loses its electricity only partially; it being discharged only from the part which was actually touched by the conductor, or those in the neighbourhood of it; so that the spark of electric fire is not so dense, nor the explosion made by parting with it so loud, from excited as from communicated electricity.

ELECTRIC substances brought into contact with excited electrics, will not destroy their electricity; whence it is that they are called non-conductors, because they will not convey or *conduct* away whatever is the cause of electric appearances in bodies.

WHEN electricity is strongly communicated to insulated animal bodies, the pulse is quickened, and perspiration increased; and, if they receive, or part with their electricity on

a sudden, a painful sensation is felt at the place of communication.

THE growth of vegetables is quickened by electricity.

No electric can be excited without producing electric appearances in the body with which it is excited, provided that body be insulated. For this insulated rubber will attract light bodies, give sparks, and make a snapping noise upon the approach of a conductor, as well as the excited electric.

IF an insulated conductor be pointed, or if a pointed conductor communicating with the earth be held pretty near it, little or no electric appearance will be exhibited; only a light will appear at each of the points, during the act of excitation, and a current of air will be sensible from off them both.

THESE two electricities, viz. that of the electric itself, and that of the rubber, though similar to, are the reverse of one another. A body attracted by the one will be repelled by the other, and they will attract, and in all respects act upon one another more sensibly than upon other bodies; so that two pieces of glass or silk, possessed of contrary electricities, will cohere firmly together, and require a considerable force to separate them.

THESE two electricities, having been first discovered by producing one of them from glass, and the other from amber, sealing-wax, sulphur, rosin, &c. first obtained the names of *vitreous* and *resinous electricity*; and it being afterwards imagined that one of them

was



was a redundancy, and the other a deficiency of a supposed electric fluid, the former (viz. that which is produced from the friction of smooth glass tubes or globes by the human hand, or a common leathern rubber) obtained the name of *positive*; and the latter (viz. that which is produced from the friction of sticks or globes of sulphur, &c. or collected from the rubber of a glass globe above mentioned) that of *negative* electricity: and these terms are now principally in use.

If a conductor, not insulated, be brought within the *atmosphere*, that is, the sphere of action, of any electrified body, it acquires the electricity opposite to that of the electrified body; and the nearer it is brought, the stronger opposite electricity doth it acquire, till the one receive a spark from the other, and then the electricity of both will be discharged.

THE electric substance which separates the two conductors, possessing these two opposite kinds of electricity, is said to be *charged*. Plates of glass are the most convenient for this purpose, and the thinner the plate, the greater charge it is capable of holding. The conductors contiguous to each side of the glass are called their *coating*.

AGREEABLE to the above-mentioned general principle, it is necessary, that one side of the charged glass have a communication with the rubber, while the other receives the electricity from the conductor, or with the

conductor, while the other receives from the rubber.

IT follows also, that the two sides of the plate thus charged are always possessed of the two opposite electricities; that side which communicates with the excited electric having the electricity of the electric, and that which communicates with the rubber, that of the rubber.

THERE is, consequently, a very eager attraction between these two electricities with which the different sides of the plate are charged; and, when a proper communication is made by means of conductors, a flash of electric light, attended with a report (which is greater or less in proportion to the quantity of electricity communicated to them, and the goodness of the conductors) is perceived between them, and the electricity of both sides is thereby discharged.

THE substance of the glass itself, in or upon which these electricities exist, is impervious to electricity, and does not permit them to unite; but if they be very strong, and the plate of glass very thin, they will force a passage through the glass. This, however, always breaks the glass, and renders it incapable of another charge.

THE flash of light, together with the explosion between the two opposite sides of a charged electric, is generally called the *electric shock*, on account of the disagreeable sensation it gives any animal, whose body is made use

use of to form the communication between them.

THIS electric shock is always found to perform the circuit from one side of the charged glass to the other by the shortest passage, through the best conductors. Common communicated electricity also observes the same rule, in its transmission from one body to another.

IT has not been found that the electric shock, takes up the least sensible space of time in being transmitted to the greatest distances.

THE electric shock, as also the common electric spark, displaces the air through which it passes; and if its passage from conductor to conductor be interrupted by non-conductors of a moderate thickness, it will rend and tear them in its passage; and in such a manner as to exhibit the appearance of a sudden expansion of the air about the center of the shock.

IF an electric shock, or strong spark, be made to pass through, or over the belly of a muscle, it forces it to contract as in a convulsion.

IF a strong shock be sent through a small animal body, it will often deprive it instantly of life.

WHEN the electric shock is very strong, it will give polarity to magnetic needles, and sometimes it reverses their poles.

GREAT shocks, by which animals are killed, are said to hasten putrefaction.

ELEC-

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ELECTRICITY and lightning are, in all respects, the same thing. Every effect of lightning may be imitated by electricity, and every experiment in electricity may be made with lightning, brought down from the clouds, by means of insulated pointed rods of metal.

PART

## P A R T III.

## THEORIES OF ELECTRICITY.

## S E C T I O N I.

OF PHILOSOPHICAL THEORIES IN GENERAL,  
AND THE THEORIES OF ELECTRICITY  
PRECEDING THAT OF DR. FRANKLIN.

ONE of the most intimate of all associations in the human mind is that of *cause* and *effect*. They suggest one another with the utmost readiness upon all occasions; so that it is almost impossible to contemplate the one, without having some idea of, or forming some conjecture about the other. In viewing the works of nature, we necessarily become first acquainted with appearances or effects. We naturally attend to the circumstances in which such appearances always arise, and cannot help considering them as the causes of those appearances. Then, considering these circumstances themselves as new appearances, we are desirous of tracing out other circumstances that gave birth to them. Thus, constantly ascending in this chain of  
causes

causes and effects, we are led, at last, to the first cause of all: and then we consider all secondary and inferior causes, as nothing more than the various methods in which the supreme cause acts, in order to bring about his great designs.

IN all science, we best ascend from particular to general. For nature exhibits nothing but particulars; and all general propositions, as well as general terms, are artificial things, being contrived for the ease of our conception and memory; in order to comprehend things clearly, and to comprise as much knowledge as possible in the smallest compass. It is no wonder then that we take pleasure in this process. Besides, we actually see in nature a vast variety of effects proceeding from the same general principles, operating in different circumstances; so that judging from appearances, that nature is every where uniform with itself, we are led, by analogy, to expect the same in all cases, and think it an argument in favour of any system, if it exhibits a variety of effects springing from a few causes. For such variety in effects, and such simplicity in causes, we generally see in nature.

HAVING discovered the cause of any appearance, it is the business of philosophy to trace it in all its effects, and to predict other similar appearances from similar previous situations of things. By this means, the true philosopher, knowing what will be the result of putting every thing, which the present system exhibits, into every variety of circumstances,

stances, is master of all the powers of nature, and can apply them to all the useful purposes of life. Thus does *knowledge*, as Lord Bacon observes, become *power*; and thus is the philosopher capable of providing, in a more effectual manner, both for his own happiness and for that of others; and thereby of approving himself a good citizen, and an useful member of society.

It is obvious, from this general view of the business of philosophy, that, in order to trace those circumstances in which any appearance in nature is certainly and invariably produced, it is chiefly useful to observe what there is *in common* in the circumstances attending similar appearances: for on those common circumstances, all that is common in the appearances must depend. And the easiest possible method, by which we can trace out the connection of causes and effects in nature, is to begin with comparing those appearances which are most similar, where the difference consists in a single circumstance; the whole effect of which, in different appearances, is thereby perfectly known. And when we have, by this means, noted the whole effect of all the separate circumstances and situations of things, we can easily judge of their effect in all possible combinations.

HENCE *analogy* is our best guide in all philosophical investigations; and all discoveries, which were not made by mere accident, have been made by the help of it. We observe a complex appearance, attended with a proportionable

tionable variety of circumstances and situations. We also see another appearance, in some respects similar, in others dissimilar; the circumstances being, likewise, similar and dissimilar in the same proportion; or we purposely vary the circumstances of the former appearance, and observe what difference it occasions. But, unless there be a very great analogy, or similarity, between them, so that the influence of a single circumstance, or of a few circumstances, can be traced separately, no probable judgment can be formed of their real operation.

BUT in all this process, a man who acts from design, and not absolutely at random, would never think of trying the influence of any circumstance in an appearance, unless, from some other analogies in nature, more or less perfect, he had formed some idea what its influence would probably be; at least, he must, from analogies in nature previously observed, have formed an idea of several possible consequences, and try which of them will really follow. That is, in other words, every experiment, in which there is any design, is made to ascertain some *hypothesis*. For an hypothesis is nothing more than a preconceived idea of an event, as supposed to arise from certain circumstances, which must have been imagined to have produced the same, or a similar effect, upon other occasions. An hypothesis absolutely verified ceases to be termed such, and is considered as a fact; though, when it has long been in an hypothetical state,

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it may continue to be called, occasionally, by the same name.

THE only danger in the use of hypotheses arises from making this transition too soon. And when an hypothesis is no longer considered as a mere probable supposition, but a real fact; a philosopher not only acquiesces in it, and thereby mistakes the cause of one particular appearance; but, by its analogies, he mistakes the cause of other appearances too, and is led into a whole system of error. A philosopher who has been long attached to a favourite hypothesis, and especially if he have distinguished himself by his ingenuity in discovering or pursuing it, will not, sometimes, be convinced of its falsity by the plainest evidence of fact. Thus both himself, and all his followers, are put upon false pursuits, and seem determined to warp the whole course of nature, to suit their manner of conceiving of its operations.

BUT, provided philosophers can be upon their guard against this species of vanity (which must be owned to be very tempting) and against the obstinacy which is the consequence of it; hypotheses, and even a great variety of them, are certainly very promising circumstances to philosophical discoveries. Hypotheses, while they are considered merely as such, lead persons to try a variety of experiments, in order to ascertain them. In these experiments, new facts generally arise. These new facts serve to correct the hypothesis which gave occasion to them. The theory, thus  
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corrected, serves to discover more new facts; which, as before, bring the theory still nearer to the truth. In this progressive state, or method of approximation, things continue; till, by degrees, we may hope that we shall have discovered all the facts, and have formed a perfect theory of them. By this perfect theory, I mean a system of propositions, accurately defining all the circumstances of every appearance, the separate effect of each circumstance, and the manner of its operation.

I HAVE dwelt so long upon this subject, because I apprehend, that electricians have generally been too much attached to their several theories, so as to have retarded the progress of real discoveries. Indeed, no other part of the whole compass of philosophy affords so fine a scene for ingenious speculation. Here the imagination may have full play, in conceiving of the manner in which an invisible agent produces an almost infinite variety of visible effects. As the agent is invisible, every philosopher is at liberty to make it whatever he pleases, and ascribe to it such properties and powers as are most convenient for his purpose. And, indeed, if he can frame this theory so as really to suit all the facts, it has all the evidence of truth that the nature of things can admit.

WITH the first electricians, electrical attraction was performed by means of *unctuous effluvia* emitted by the excited electric. These were supposed to fasten upon all bodies in  
 2 their

their way, and to carry back with them all that were not too heavy. For, in that age of philosophy, all effluvia were supposed to return to the bodies from which they were emitted; since no person could, otherwise, account for the substance not being sensibly wasted by the constant emission. When these light bodies, on which the unctuous effluvia had fastened, were arrived at the excited electric, a fresh emission of the effluvia was supposed to carry them back again. But this effect of the effluvia was not thought of, till electrical repulsion had been sufficiently observed.

WHEN the Newtonian philosophy had made some progress, and the extreme subtilty of light, and other effluvia of bodies, was demonstrated; so that philosophers were under no apprehension of bodies being wasted by continual emission, the doctrine of *the return of the effluvia* was universally given up as no longer necessary; and they were obliged to acquiesce in the unknown principles of attraction and repulsion, as supposed to be properties of certain bodies, communicated to them by the Divine Being, the mechanical cause of which they scarce attempted to explain.

WHEN Mr. Du Fay discovered the two opposite species of electricity, which he termed the *vitreous* and *resinous* electricity, he necessarily formed the idea of *two distinct electric fluids*, repulsive with respect to themselves, and attractive of one another. But he had no

idea of both species being actually concerned in every electrical operation, and that glass or rosin alone always produced them both. This theory, therefore, was as simple in its application as the other.

WHILE nothing more was known of electricity but attraction and repulsion, this general theory was sufficient. The general attraction of all bodies to all bodies was called (and by some absurdly enough supposed to be accounted for by) gravitation, and many superficial philosophers thought they had given a very good account of electricity, cohesion, and magnetism, by calling them particular species of attraction peculiar to certain bodies.

BUT when electricity began to show itself in a greater variety of appearances, and to make itself sensible to the smell, the sight, the touch, and the hearing: when bodies were not only attracted and repelled, but made to emit strong sparks of fire, attended with a considerable noise, a painful sensation, and a strong phosphoreal smell; electricians were obliged to make their systems more complex, in proportion as the facts were so. It was then generally supposed, that the matter of the electric fluid was the same with the chymical principle of fire; though some thought it was a fluid *sui generis*, which very much resembled that of fire; and others, with Mr. Boulanger at their head, thought that the electric fluid was nothing more than the finer parts of the atmosphere, which crowded upon the  
the

the surfaces of electric bodies, when the grosser parts had been driven away by the friction of the rubber.

THE great difficulty common to all these theories was to ascertain the direction of the electric matter. It is no wonder that, when electrical appearances were first observed, all electric powers were supposed to reside in, and therefore to proceed from the excited electric. Consequently, the electric spark was first imagined to be darted from the electrified body towards any conductor that was presented to it. It was never imagined there could be any difference in this respect whether it was amber, glass, sealing-wax, or any thing else that was excited. Nothing was thought to be more evident to the senses than this progress of the electric matter: what then must have been the astonishment of all electricians, when they first observed electric appearances at an insulated rubber; at the same time that it was demonstrated, that the action of the rubber did not *produce*, but only *collect* the electric fluid.

IN this case, the current could not have been supposed to flow both from the conductor and the rubber; and yet the first appearances were the same. To provide a supply of the electric matter, they were obliged to suppose that, notwithstanding appearances were nearly the same, the electric fluid was really received by the electrified body in the one case, and emitted by it in the other. But now, being obliged to give up the argument

for the manner of its progress from the evidence of sight, they were at a loss whether, in the usual method of electrifying by excited glass, the fluid proceeded from the rubber to the conductor, or from the conductor to the rubber; and nothing was found to obviate these difficulties, till an excellent theory of positive and negative electricity was suggested by Dr. Watson, and digested and illustrated by Dr. Franklin.

It was soon found, that the electricity, at the rubber was the reverse of that at the conductor, and in all respects the same with that which had before been produced by the friction of sealing-wax, sulphur, rosin, &c. Seeing, therefore, that both the electricities, as they had heretofore been called, were produced at the same time, by one and the same electric, and by the same friction, all electricians, and among the rest Mr. Du Fay himself, concluded, that they were both modifications of one and the same fluid; and the old doctrine of the different electricities was universally discarded.

THE accidental discovery of the Leyden phial most clearly demonstrated the imperfection of all the theories preceding that of positive and negative electricity, by exhibiting an astonishing appearance, which no electricians, with the help of any theory, could have foreseen, and of which they could have formed no idea, *a priori*.

UPON this great event, new theories of electricity multiplied apace, so that it would be to

no

no purpose to enumerate them all. Indeed many of them were no more than the beings of a day. For no sooner were they started, but the authors themselves, upon the appearance of some new fact, saw reason to new model, or entirely reject them. I shall, therefore, content myself with giving the outlines of some of the principal theories of electricity, which have their adherents at present, without considering whether they took their rise before or after this discovery,

WITH some, and particularly Mr Wilson, the chief agent in all electrical operations is Sir Isaac Newton's ether; which is more or less dense in all bodies, in proportion to the smallness of their pores, except that it is much denser in sulphureous and unctuous bodies \*. To this ether are ascribed the principal phenomena of attraction and repulsion, whereas the light, the smell, and other sensible qualities of the electric fluid are referred to the grosser particles of bodies, driven from them by the forcible action of this ether. Many phenomena in electricity are also attempted to be explained by means of a subtile medium, at the surface of all bodies, which is the cause of the refraction and reflection of the rays of light, and also resist the entrance and exit of this ether †. This medium, he says, extends to a small distance from the body, and is of the same nature with what is called the electric fluid. On the surface of

\* Wilson's Dissertation, p. 5. † Hoadley and Wilson, p. 55.

conductors this medium is rare, and easily admits the passage of the electric fluid; whereas on the surface of electrics it is dense, and resists it. This medium is rarefied by heat, which converts non-conductors into conductors \*. On this theory I shall make no particular remarks, because I cannot say that I clearly comprehend it.

BUT the far greater number of philosophers suppose, and with the greatest probability, that there is a fluid *jui generis* principally concerned in the business of electricity. They seem, however, though perhaps without reason, entirely to overlook Sir Isaac Newton's ether; or if they do not suppose it to be wholly unconcerned, they allow it only a secondary and subordinate part to act in this drama. And among those who suppose a fluid *jui generis*, there is a great diversity of opinions about the mode of its existence, and the manner of its operation.

THE ingenious Abbé Nollet, whose theory has been more the subject of debate than all the other theories before Dr. Franklin's, supposes that, in all electrical operations, the fluid is thrown into two opposite motions; that the *affluence* of this matter drives all light bodies before it, by impulse, upon the electrified body, and its *effluence* carries them back again. But he seems very much embarrassed in accounting for facts where both these currents must be considered, at the same

• Hoadley and Wilson, p. 78.



time that he is obliged to find expedients to prevent their impeding the effects of each other. To obviate this great difficulty, he supposes, that every excited electric, and likewise every body to which electricity is communicated, has two orders of pores, one for the emission of the effluvia, and the other for the reception of them. A man of less ingenuity than the Abbé could not have maintained himself in such a theory as this; but, with his fund of invention, he was never at a loss for resources upon all emergencies, and in his last publication appears to be as zealous for this strange hypothesis as at the first.

He more than once requested a deputation of the members of the Academy of Sciences, to be witnesses of some experiments, in which, he thought, there was a visible effluence of the electrical effluvia from the conductor, both to the globe at one of its extremities, and to any non-electric presented to it at the other; and their testimony was signed and registered in proper form \*. But it does not seem to the honour of Mr. Nollet, or those gentlemen of the Academy, to be so very positive in a matter which does not admit of the evidence of sense.

THE Abbé's confidence upon this subject is very remarkable. These effects, says he, well considered, and reviewed a thousand times, in the course of thirty years, in which

\* *Leçons de Physique*, p. 368. 395.

I have applied to electricity, make me say with confidence, that those pencils of rays are currents of electric matter, which fly from the conductor towards the excited globe. This is so evident, that I would freely appeal to the ocular testimony of any unprejudiced person, who should see the experiments which I have recited. But, says he, the fact in question is contrary to a system of electricity, which some persons persist in maintaining. They have the assurance to tell me, that the matter of the luminous pencil, in my experiment, moves in a direction quite opposite to that which I suppose; that it proceeds from the excited globe, and is from thence thrown upon any non-electric within its reach \*. In another place, he says, that the principle of simultaneous effluences and affluences is by no means a *system*, but a *fact* well proved †.

THE Abbé Nollet proposes an hypothesis to explain the difference between common electricity and the electric shock. All the effects of common electricity, he says, plainly show, that the electric matter is animated with a progressive motion, which really carries it forwards; whereas the remarkable case of the electric shock appears to be an instantaneous percussion, which the contiguous parts of the same matter communicate to one another, without being displaced. Sound and wind, he says, are motions of the air; but would a philosopher, be permitted to take the one for

\* Leçons de Physique, p. 363.

† Lettres sur l'Électricité, p. 98.

the other, in measuring their velocity or extent. But this comparison is by no means just \*.

It must be acknowledged, that, far the greater part of the Abbé Nollet's arguments in favour of his doctrine of effluences and affluences are very unsatisfactory, and that his method of accounting for electrical attraction and repulsion, with other phenomena in electricity, by means of it, is more ingenious than solid. It is a great pity that this truly excellent philosopher had not spent more time in diversifying facts, and less in refining upon theory. But it is in some measure the natural fault of a disposition to philosophize.

MR. DU TOUR improves upon this hypothesis of the Abbé Nollet, by supposing that there is a difference between the affluent and effluent current; and that the particles of the fluid are thrown into vibrations of different qualities, which makes one of these currents more copious than the other, according as sulphur or glass is used. Difficult as it is to form any idea of this hypothesis, the author appears very much attached to it, and has no doubt of its accounting for all electrical appearances.

\* Leçons de Physique, p 293.

## SECTION

## SECTION II.

## THE THEORY OF POSITIVE AND NEGATIVE ELECTRICITY.

THE English philosophers, and perhaps the greater part of foreigners too, have now generally adopted the theory of *positive* and *negative* electricity. As this theory has been extended to almost all the phenomena, and is the most probable of any that have been hitherto proposed to the world, I shall give a pretty full account of it, and show how it agrees with all the propositions of the last part, to which it has hitherto been applied.

THIS theory generally goes by the name of Dr. Franklin, and there is no doubt of his right to it; but justice requires that I distinctly mention the equal, and, perhaps, prior claim of Dr. Watſon, to whom I have before said it had occurred. Dr. Watſon showed a series of experiments to confirm the doctrine of *plus* and *minus* electricity to Martin Folkes, Esq. then president, and to a great number of fellows of the Royal Society, so early as the beginning of the year 1747, before it was known in England that Dr. Franklin had discovered the same thing in America. See the Philosophical Transactions, vol. xlv. p. 739; and vol. xlv. p. 93—101. Dr. Franklin's paper,

paper, containing the same discovery, was dated at Philadelphia, June the 1st, 1747.

ACCORDING to this theory, all the operations of electricity depend upon one fluid *sui generis*, extremely subtle and elastic, dispersed through the pores of all bodies; by which the particles of it are as strongly attracted, as they are repelled by one another.

WHEN the equilibrium of this fluid in any body is not disturbed; that is, when there is in any body neither more nor less of it than its natural share, or than that quantity which it is capable of retaining by its own attraction, it does not discover itself to our senses by any effect. The action of the rubber upon an electric disturbs this equilibrium, occasioning a deficiency of the fluid in one place, and a redundancy of it in another.

THIS equilibrium being forcibly disturbed, the mutual repulsion of the particles of the fluid is necessarily exerted to restore it. If two bodies be both of them overcharged, the electric atmospheres (to adopt the ideas of all the patrons of this hypothesis before Mr. Æpinus) repel each other, and both the bodies recede from one another to places where the fluid is less dense. For, as there is supposed to be a mutual attraction between all bodies and the electric fluid, electrified bodies go along with their atmospheres. If both the bodies be exhausted of their natural share of this fluid, they are both attracted by the denser fluid, existing either in the atmosphere  
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contiguous to them, or in other neighbouring bodies; which occasions them still to recede from one another, as much as when they were overcharged.

SOME of the patrons of the hypothesis of positive and negative electricity conceive otherwise of the immediate cause of this repulsion. They say that, as the denser electric fluid, surrounding two bodies negatively electrified, acts equally on all sides of those bodies, it cannot occasion their repulsion. Is not the repulsion, say they, owing rather to an accumulation of the electric fluid on the surfaces of the two bodies; which accumulation is produced by the attraction of the bodies, and the difficulty the fluid finds in entering them? This difficulty in entering is supposed to be owing, chiefly, to the *air* on the surface of bodies, which is probably a little condensed there; as may appear from Mr. Canton's experiment above mentioned on the double barometer.

LASTLY, if one of the bodies have an overplus of the fluid, and the other a deficiency of it, the equilibrium is restored with great violence, and all electrical appearances between them are more striking.

THE influence of *points* in drawing or throwing off the electric fluid has not been quite satisfactorily accounted for upon any hypothesis, but it is as agreeable to this as any other. As it is evident that every electric atmosphere meets with some resistance, both in entering and quitting any body, what-  
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ever be the cause of that resistance, it is natural to suppose, that it must be least at the points of bodies where there are fewer particles of the body (on which the resistance depends) opposed to its passage, than at the flat parts of the surface, where the resisting power of a greater number of particles is united.

THE *light* which is visible in electrical appearances is generally supposed to be part of the composition of the electric fluid, which appears when it is properly agitated. But this supposition concerning electric light is not necessary to the general hypothesis. It may be supposed, upon this as well as Mr. Wilson's theory, that the light, and the phosphoreal smell, in electrical experiments arise from particles of matter much grosser than the proper electric fluid, but which may be driven from bodies by its powerful action.

THE *sound* of an electrical explosion is certainly produced by the air being displaced by the electrical fluid, and then suddenly collapsing, so as to occasion a vibration, which diffuses itself every way from the place where the explosion was made. For in such vibrations sound is known to consist.

BUT the chief excellence of this theory of positive and negative electricity, and that which gave it the greatest reputation, is the easy explication which it suggests of all the phenomena of the Leyden phial. This fluid is supposed to move with the greatest ease in bodies which are conductors, but with extreme difficulty in electrics *per se*; inasmuch that

that glass is absolutely impermeable to it. It is moreover supposed, that all electrics (and particularly glass) on account of the smallness of their pores, do at all times contain an exceedingly great, and always an equal quantity of this fluid; so that no more can be thrown into one part of any electric substance, except the same quantity go out at another, and the gain be exactly equal to the loss. These things being previously supposed, the phenomena of charging and discharging a plate of glass admit of an easy solution.

IN the usual manner of electrifying, by a smooth glass globe, all the electric matter is supplied by the rubber from all the bodies which communicate with it. If it be made to communicate with nothing but one of the coatings of a plate of glass, while the conductor communicates with the other, that side of the glass which communicates with the rubber must necessarily be exhausted, in order to supply the conductor, which must convey the whole of it to the side with which it communicates. By this operation, therefore, the electric fluid becomes almost entirely exhausted on one side of the plate, while it is as much accumulated on the other; and the discharge is made by the electric fluid rushing, as soon as an opportunity is given it, by means of proper conductors, from the side which was overloaded to that which was exhausted.

It is not necessary, however, to this theory, that the very same individual particles of electric



tric matter which were thrown upon one side of the plate, should make the whole circuit of the intervening conductors, especially in very great distances, so as actually to arrive at the exhausted side. It may be sufficient to suppose, as was observed before, that the additional quantity of fluid displaces and occupies the space of an equal portion of the natural quantity of fluid belonging to those conductors in the circuit, which lay contiguous to the charged side of the glass. This displaced fluid may drive forwards an equal quantity of the same matter in the next conductors; and thus the progress may continue, till the exhausted side of the glass is supplied by the fluid naturally existing in the conductors contiguous to it. In this case the motion of the electric fluid in an explosion will rather resemble the vibration of the air in sounds, than a current of it in winds.

It will easily be acknowledged, that while the substance of the glass is supposed to contain as much as it can possibly hold of the electric fluid, no part of it can be forced into one of the sides, without obliging an equal quantity to quit the other side; but it may be thought a difficulty upon this hypothesis, that one of the sides of a glass plate cannot be *exhausted*, without the other receiving more than its natural share, particularly as the particles of this fluid are supposed to be repulsive of one another. But it must be considered, that the attraction of the glass is sufficient to retain even the large quantity of the electric fluid

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which is natural to it, against all attempts to withdraw it, unless that eager attraction can be satisfied by the admission of an equal quantity from some other quarter. When this opportunity of a supply is given, by connecting one of the coatings with the rubber, and the other with the conductor, the two attempts to introduce more of the fluid into one of the sides, and to take from it on the other, are made, in a manner, at the same instant. The action of the rubber tends to disturb the equilibrium of the fluid in the glass, and no sooner has a spark quitted one of the sides, to go to the rubber, but it is supplied by the conductor on the other; and the difficulty with which these additional particles move in the substance of the glass effectually prevents its reaching the opposite exhausted side; near as the two sides are to one another, and eager as is the endeavour of the fluid to go whither it is so strongly attracted.

It is not said, however, but that either side of the glass may give or receive a *small quantity* of the electric fluid, without altering the quantity on the opposite side. It is only a very considerable part of the charge that is meant, when one side is said to be filled, while the other is exhausted.

It is a little remarkable, that the electric fluid, in this, and in every other hypothesis, should so much resemble the ether of Sir Isaac Newton in some respects, and yet differ from it so essentially in others. The electric fluid is supposed to be, like ether, extremely subtile  
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and elastic, that is, repulsive of itself; but, instead of being, like the ether, repelled by all other matter, it is strongly attracted by it; so that, far from being, like the ether, rarer in the small than in the large pores of bodies, rarer within the bodies than at their surfaces, and rarer at their surfaces than at any distance from them; it must be denser in small than in large pores, denser within the substance of bodies than at their surfaces, and denser at their surfaces than at a distance from them. But no other property can account for the extraordinary quantity of this fluid contained within the substance of electrics *per se*, or for the common atmospheres of all excited and electrified bodies.

To account for the attraction of light bodies, and other electrical appearances, in air of the same density with the common atmosphere, when glass (which is supposed to be impermeable to electricity) is interposed; it is conceived, that the addition or subtraction of the electric fluid, by the action of the excited electric, on one side of the glass, occasions (as in the experiment of the Leyden phial) a subtraction or addition of the fluid on the opposite side. The state of the fluid, therefore, on the opposite side being altered, all light bodies within the sphere of its action must be affected, in the very same manner as if the effluvia of the excited electric had actually penetrated the glass, according to the opinion of all electricians before Dr. Franklin.

THE manner in which *clouds* acquire their positive or negative electricity is not determined, according to this, or any other theory, with sufficient certainty. Mr. Canton's conjecture is, that the air resembles the tourmalin, and, consequently, acquires its electricity by heating or cooling; but whether it gains or loses, the electric fluid in either state must be determined by experiment. Signior Beccaria's theory of the electricity of the clouds has been related at large.

THIS hypothesis of positive and negative electricity has been adopted, and, in some measure, rendered more systematical by Mr. Æpinus, in his elaborate treatise entitled, *Tentamen Theoriæ Electricitatis et Magnetismi*.

HE has extended the property of impermeability to air and all electrics as well as glass, and defined it in a better manner; supposing impermeability to consist in the great difficulty with which electric substances admit the electric fluid into their pores, and the slowness with which it moves in them. Moreover, in consequence of this impermeability of air to the electric fluid, he denies the reality of electric atmospheres, and thinks, as was observed before, that Dr. Franklin's theory will do much better without them.

HE thinks that all the particles of matter must repel one another: for that, otherwise (since all substances have in them a certain quantity of the electric fluid, the particles of which repel one another, and are attracted  
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by all other matter) it could not happen, that bodies in their natural state, with respect to electricity, should neither attract nor repel one another.

HE that reads the first chapter, as well as many other parts of his elaborate treatise above mentioned, may save himself a good deal of time and trouble by considering, that the result of many of his reasonings and mathematical calculations cannot be depended upon; because he supposes the repulsion or elasticity of the electric fluid to be in proportion to its condensation; which is not true, unless the particles repel one another in the simple reciprocal ratio of their distances, as Sir Isaac Newton has demonstrated, in the second book of his Principia.

MR. WILCKE, as well as Mr. Æpinus, adopts all the general principles of Dr. Franklin's theory of positive and negative electricity, but thinks that no experiments which have hitherto been made show which of the electricities is positive and which negative. Supposing, however, what is called positive to be really so, and that smooth glass, for instance, rubbed upon sulphur attracts the electric fluid from it, he would account for it upon the same principles whereby water stands in drops on rough surfaces, but is diffused on smooth ones. The electric fluid, he would suppose, is more strongly attracted by the smooth surface of the glass, and therefore diffuses itself over it, while it retreats from elec-

tries of rougher surfaces \*. But this explanation, I imagine, will give little satisfaction to sceptical electricians.

MR. WILCKE acknowledges there is great difficulty in accounting for the repulsive power of bodies electrified negatively, and thinks that it obliges us to suppose the mutual repulsion of all homogeneous matter. Mr. Waitz, he says, was of the same opinion. According to him, therefore, bodies which have too great a proportion either of the electric fluid, or of their proper constituent matter, must avoid one another. In the former case, by the repulsion of the electric fluid, in the latter, by the repulsion of the constituent parts of the bodies. Mr. Wilcke observes upon this subject, that the attraction of light bodies to negative electrics cannot be owing to the repulsive power of the electric fluid in the neighbouring air, driving them, or the electric matter in them to the place where there is a want of it; because the velocity ought to decrease as it recedes from the impulsive power: whereas it is accelerated, as if it were attracted by the negative electric †.

BUT to this it may be replied, that a succession of impulses, though every subsequent one should be weaker than the preceding, will produce an accelerated motion. Besides, the nearer the light body is to the negative elec-

\* Wilcke, p. 65.

† Ibid. p. 15, and Remarks on Franklin's Letters, p. 270.

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ric, the nearer it is to the point where the equilibrium of the fluid is most destroyed; or the less force there is on the side of the electric to balance the force that drives the light body towards it, and therefore the impulses themselves must increase.

MR. WILCKE, whose treatise on the two electricities is admirable, both for its materials and methodical arrangement of them, distinguishes three causes of excitation, viz. *warming, liquefaction, and friction*; and he advises, that we carefully distinguish between *spontaneous* and *communicated* electricity. By the former he means that which is the result of the apposition, or mutual action of two bodies; in consequence of which, one of them is left positively electric and the other negative. Whereas *communicated* electricity is that which is superinduced upon a body, or part of a body, electric or non-electric, without its being previously heated, melted, or rubbed; or without any mutual action between it and any other body. This distinction is, in general, very obvious; but Mr. Wilcke defines it more accurately than it had been done before, and mentions several cases in which they are often confounded.

SIGNIOR BECCARIA admits the theory of positive and negative electricity, though he explains some electrical phenomena in a manner different from the other patrons of that system.

HE supposes that electrified bodies move to one another only in the act of giving and re-

ceiving the electric fluid \*: this effect being produced by the electric matter making a vacuum in its passage, and the contiguous air afterwards collapsing, and thereby pushing the bodies together †. This vacuum, he says, is very observable upon great explosions of thunder, when animals have been struck dead without being touched with the lightning; a vacuum being only suddenly made near them, and the air immediately rushing out of their lungs to fill it, whereby they are left flaccid and empty; whereas when persons are properly killed by lightning, their lungs are found distended ‡.

IN confirmation of this hypothesis, he says, that less motion is given to bodies by electricity, as the air is excluded from them, and that in vacuo no motion at all can be given to them ||. He also says, that no electric light is visible in a barometer in which there is a perfect vacuum: whence he infers, that electric light is visible only by means of some vibrations which it excites in the air §.

To account for the collection or dissipation of electricity by points, he says that the electric fluid appears, from experiments, to move with the greatest violence in the smallest bodies. All electrical appearances will, therefore, be most sensible at the points of bodies; and, consequently, it will be soonest dissipated

\* *Lettere dell' elettricismo*, p. 36.

† *Ibid.* p. 42.

|| *Ibid.* p. 48.

‡ *Ibid.* p. 41.

§ *Ibid.* p. 50.

there.



there. But this does not seem to touch the real difficulty.

DR. FRANKLIN, the author of this excellent theory of positive and negative electricity, with a truly philosophical greatness of mind, to which few persons have ever attained, always mentions it with the utmost diffidence. Every appearance, says he, which I have yet seen, in which glass and electricity are concerned, are, I think, explained with ease by this hypothesis. Yet, perhaps, it may not be a true one, and I shall be obliged to him who affords me a better \*.

It is no wonder, indeed, that this excellent philosopher should treat even his own hypothesis with such indifference, when he had so just a sense of the nature, use, and importance of all hypotheses. Nor is it, says he, of much importance to us, to know the manner in which nature executes her laws. It is enough if we know the laws themselves. It is of real use to us to know that china left in the air, unsupported, will fall and break; but how it comes to fall, and why it breaks, are matters of speculation. It is a pleasure indeed to know them, but we can preserve our china without it †.

THE great merit of this writer as an electrician stands independent of all hypotheses, upon the firm basis of the discovery of many new and import facts, and, what is more,

\* Franklin's Letters, p. 78.

† Ibid. p. 59.

#### 40 POSITIVE AND NEGATIVE, &c.

applied to the greatest uses. Supposing him, for instance, to have been mistaken in his account how the clouds come to be possessed of electricity, must not all the world acknowledge themselves indebted to him for the discovery of the sameness of the electric fluid and the matter of lightning; and especially for a certain method of preserving their buildings and persons from the fatal effects of thunder storms.

#### SECTION

## S E C T I O N III.

## OF THE THEORY OF TWO ELECTRIC FLUIDS.

CONVINCED, as the reader may have perceived that I am, of the usefulness of various theories, as suggesting a variety of experiments, which lead to the discovery of new facts; he will excuse me, if I recall his attention to the old theory of vitreous and resinous electricity, as it was first suggested by Mr. Du Fay, upon his discovery of the different properties of excited glass, and excited amber, sulphur, rosin, &c. and as it has been new modelled by Mr. Symmer. To show my absolute impartiality, I shall, notwithstanding the preference I have given to Dr. Franklin's theory, endeavour to represent this to as much advantage as possible, and even to do it more justice than has yet been done to it, even by Mr. Symmer himself; who, as I observed before, has fallen into some mistakes in his application of it. Indeed, hitherto very little pains has been taken with this theory, nor has it been extended to any great variety of phenomena.

LET us suppose then, that there are two electric fluids, which have a strong chymical affinity with each other, at the same time that  
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the particles of each are as strongly repulsive of one another. Let us suppose these two fluids, in some measure, equally attracted by all bodies, and existing in intimate union in their pores, and while they continue in this union to exhibit no mark of their existence. Let us suppose that the friction of any electric produces a separation of these two fluids, causing (in the usual method of electrifying) the vitreous electricity of the rubber to be conveyed to the conductor, and the resinous electricity of the conductor to be conveyed to the rubber. The rubber will then have a double share of the resinous electricity, and the conductor a double share of the vitreous; so that, upon this hypothesis, no substance whatever can have a greater or less quantity of electric fluid at different times. The quality of it only can be changed.

THE two electric fluids, being thus separated, will begin to show their respective powers, and their eagerness to rush into reunion with one another. With whichever of these fluids a number of bodies are charged, they will repel one another, they will be attracted by all bodies which have a less share of that particular fluid with which they are loaded, but will be much more strongly attracted by bodies which are wholly destitute of it, and loaded with the other. In this case they will rush together with great violence.

UPON this theory, every electric spark consists of both the fluids rushing contrary ways,  
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and making a double current. When, for instance, I present my finger to a conductor loaded with vitreous electricity, I discharge it of part of the vitreous, and return as much of the resinous, which is supplied to my body from the earth. Thus both the bodies are un-electrified, the balance of the two powers being perfectly restored.

WHEN I present the Leyden phial to be charged, and, consequently, connect the coating of one of its sides with the rubber, and that of the other with the conductor, the resinous electricity of that side which is connected with the conductor is transmitted to that which is connected with the rubber, which returns an equal quantity of its vitreous electricity; so that all the vitreous electricity is conveyed to one of the sides, and all the resinous to the other. These two fluids, being thus separated, attract one another very strongly through the thin substance of the intervening glass, and rush together with great violence, whenever an opportunity is presented, by means of proper conductors. Sometimes they will force a passage through the substance of the glass itself; and, in the mean time, their mutual attraction is stronger than any force that can be applied to draw away either of the fluids separately.

HAVING stated the general principles of this hypothesis of two fluids, I shall now enter into a brief comparison of it with that of a single fluid, as explained by the mode of positive and negative electricity; that we may see

see which of them will account for the same facts in the easier manner and more agreeable to the analogy of nature in other respects. For, allowing that no fact can be shown to be absolutely inconsistent with either of them; yet, certainly, that will be judged preferable, which is attended with the least difficulty in conceiving of its mode of operation.

IN the first place, the supposition itself, of two fluids, is not quite so easy as that of one, though it is far from being disagreeable to the analogy of nature, which abounds with affinities, and in which we see innumerable cases of substances formed, as it were to unite with and counteract one another. Here, likewise, agreeable to the theory of two electric fluids, while those substances are in union, we see nothing of their separate and peculiar powers, though they be ever so remarkable. What, for instance, do we see of the striking properties of the *acid* and *alkali* while they are united in a neutral salt? What powers in nature are more formidable than the vitreolic acid, and phlogiston, and what more innocent than common sulphur, which is a composition of them both, and from which the action of fire separates them.

THE two fluids being supposed, the double current from the rubber to the conductor and from the conductor to the rubber is an easy and necessary consequence. For if, upon the common supposition, the action of the rubber puts a single fluid into motion in one direction,

tion, we might expect that, if there were two fluids, which counteracted each other, they would, by the same operation, be made to move in contrary directions. And a person who has been used to conceive that a single fluid may be made to move either way, viz. from the conductor to the rubber, or from the rubber to the conductor, at pleasure, according as a rough or a smooth globe is used, can have much less objection to this part of the hypothesis.

ADMITTING then this different action of the rubber and the electric upon the two different fluids, the manner of conveying electric atmospheres, or powers to bodies is the same on this as on any other theory; and it is apprehended, that the phenomena of negative electricity are more easily conceived by the help of a real fluid, than by no fluid at all. Indeed Dr. Franklin himself ingenuously acknowledges, that he was a long time puzzled to account for bodies that were negatively electrified repelling one another; whereas Mr. Du Fay, who observed the same fact, had no difficulty about it, supposing that he had discovered another electricity, similar, with respect to the properties of elasticity and repulsion, to the former.

By this double action of the rubber, the method of charging a plate of glass is exceedingly easy to conceive. Upon this hypothesis, all the vitreous electricity quits its union with the resinous on the side communicating with the rubber, and is brought over  
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to the side communicating with the conductor ; which, by the same operation, had been made to part with its resinous electricity in return.

ALL the vitreous electricity being thus brought to one side of the plate of glass, and all the resinous to the other, the phenomena of the plate while standing charged, or when discharged, are, perhaps, more free from all difficulty than upon any other hypothesis. When one of the sides of the glass is conceived to be loaded with one kind of electricity, and the other side with the other kind ; the strong affinity between them, whereby they attract each other with a force proportioned to their nearness, immediately supplies a satisfactory reason, why so little of either of the fluids can be drawn from one of the sides without communicating as much to the other. Upon this supposition, that consequence is perhaps more obvious than upon the supposition of one half of the glass being crowded with the electric matter, and the other half exhausted. In the former case, every attempt to withdraw the fluid from one of the sides is opposed by the more powerful attraction of the other fluid on the opposite side. On the other hypothesis, it is only opposed by the attraction of the empty pores of the glass.

LASTLY, the explosion upon the discharge of the glass has as much the appearance of two fluids rushing into union, in two opposite directions, as of one fluid, proceeding only



only in one direction. The same may be said of the appearance of every common electric spark, in which, upon this hypothesis, there is always supposed to be two currents, one from the electric, or the electrified body, and the other to it.

I do not say that the bur which is usually seen on both sides of a quire of paper pierced by an electric explosion, and the current of air flowing from the points of all bodies electrified negatively as well as positively, are material objections to the doctrine of a single fluid. I have even shown how they may be explained in a manner consistent with it; but upon the supposition of two fluids, and two currents, the difficulty of accounting for these facts would hardly have occurred.

THE phenomena of discharging a plate of glass, upon the hypothesis of two fluids, are indeed injudiciously explained by Mr. Symmer; who supposes that the two fluids do not always make the whole circuit of the intervening conductors, but enter them, more or less, from each side of the plate, according to the strength of the charge. But upon this supposition, the fire of the smallest charge performs the whole circuit, as well as the fire of the greatest, in order to restore the equilibrium of the two fluids on each side of the glass.

It is almost needless to observe, that the influence of points is attended with exactly the same difficulty upon this theory as upon the other. It is equally easy, or equally difficult,

ficult, to suppose one fluid to enter and go out at the point of an electrified conductor, at different times, as to suppose that, of two fluids, one goes out, and the other goes in, at the same time.

THAT bodies immersed in electric atmospheres must acquire the contrary electricity, is quite as easy to conceive upon this, as upon any other hypothesis. For, in this case, suppose the electrified body to be possessed of the vitreous electricity, all the vitreous electricity of the body which is brought near it will be driven backwards, to the more distant parts, and all the resinous electricity will be drawn forwards. And when the attraction between the two electricities, in these different bodies, is so great as to overcome the opposition to their union, occasioned by the attraction of the bodies that contained them, the form of their surfaces, and the resistance of the interposing medium, they will rush together; an electric spark will be visible between them, and the electricity of both will appear to be discharged; the prevailing electricity of each being saturated with an equal quantity of the opposite kind, from the other body.

THIS hypothesis will likewise easily account for the difficulty of charging a very thick plate of glass, and the impossibility of charging it beyond a certain thickness: for these fluids at a greater distance, will attract one another less forcibly; and at a certain still greater distance will not attract at all.

HAVING

HAVING given the most favourable view that I can of this hypothesis of two electric fluids, I shall, with the same fairness, make the best answer I am able to the principal objection that will probably be made to it.

IF it be asked, why the two fluids, meeting on the surface of the globe, or in the electric explosion, do not unite, by means of their strong affinity, and make no farther progress; it may be answered, that the attraction between all other bodies and the particles of both these fluids may be supposed to be, at least, as strong as the affinity between the fluids themselves; so that the moment that any body is dispossessed of one, it may recruit itself, to its usual point of saturation, from the other.

BESIDES, in whatever manner it be that one of the electric fluids is dislodged from any body (since, upon every theory, the two electricities are always produced at the same time) the opposite electricity will, by the same action, be dislodged from the other substance. And (as upon the common theory) whatever it be that dislodges the fluid from any substance, it will be sufficient to prevent its return; consequently, supposing both the substances necessarily to have a certain proportion of electric matter, each may be immediately supplied from that which was dislodged from the other.

THE rubber, therefore, at the time of excitation, gives its vitreous electricity to that part of the smooth glass against which it has

been pressed, and takes an equal quantity of the resinous in return. The glass, being a non-conductor, does not allow this additional quantity of vitreous electricity to enter its substance. It is therefore diffused upon the surface, and, in the revolution of the globe, is carried to the prime conductor. There (as in the experiments begun by Mr. Canton, and prosecuted by Mr. Wilcke, &c.) it repels the vitreous, and violently attracts the resinous electricity; and (the points of the conductor favouring the mutual transition) the vitreous, which abounds upon the globe, passes to the conductor; and the resinous, which abounds on the nearest parts of the conductor rushes upon the globe. There it mixes with, and saturates what remained of the vitreous electricity, on the part on which it flows, and thereby reduces it to the same state in which it was before it was first excited. Every part of the surface of the globe performs the same office, first exchanging electricities with the rubber, and then with the conductor.

THE solution of this difficulty will likewise solve that of the electric explosion, in which there is a collision, as it were, of the two fluids, while yet they completely pass one another. For still each surface of the glass may be supposed to require its certain portion of electric matter, and therefore cannot part with one sort without receiving an equal quantity of the other. It must be considered also, that the air, through which these fluids pass, has already its natural quantity of electricity,  
so

so that being fully saturated, it can contain no more; and the two fluids only rush to the places from whence they had been forcibly dislodged, and where the greater body of the opposite fluid waits to embrace them.

MR. SYMMER's hypothesis of a double current differed in some respects from that of the Abbé Nollet. The Abbé, however, according to his usual candour, speaks of him with the highest respect; at the same time, he still appears an advocate for his old favourite hypothesis.

MR. CIGNA, who pursued the experiments above recited of Mr. Symmer, observes, with respect to his theory; that it is not contradicted by any phenomena that are yet known, and that it suits some of them in a peculiarly clear and elegant manner; particularly every thing relating to charging and discharging a plate of glass; all the experiments which seem to show a mutual attraction between the two electricities, when they are kept asunder; and that curious experiment above mentioned of Signior Beccaria, of discharging a plate of glass suspended by a silken string, without either touching or moving the plate. Yet, upon the whole, he declares in favour of Dr. Franklin's theory of positive and negative electricity, on account of its admirable simplicity, and because philosophers ought not to multiply causes without necessity.

DR. FRANKLIN's theory, he says, completely solves all the cases of the two electricities destroying one another when they are

mixed; but doth not so clearly account for their attracting, and counteracting one another when they are separate. He concludes with saying, he doth not chuse to say much on so very obscure a question, which has divided the opinions of very great men; and that any hypothesis of the two electricities, which will account for the destruction of all the signs of electricity when they are united, and their mutual attraction when they are separate, will equally suit all the phenomena that have been discovered.

I HAVE taken a little pains with this theory, because I thought it had been, hitherto, too much overlooked, and that sufficient justice had not been done to it, even by those who proposed it. For the future, I hope it will be seen to more advantage, and appear a little more respectable among its sister hypotheses; and then, *valeat quantum valere potest*. If any electrician will favour me with the communication of any other theory, not obviously contradicted by facts, I shall think myself obliged to him, and shall think I do a piece of real service to the science in the publication of it. If more persons favour me with more different theories, I should think my book, as far as theories are of any use, so much the more valuable.

## P A R T IV.

## DESIDERATA IN THE SCIENCE OF ELECTRICITY, AND HINTS FOR THE FARTHER EXTENSION OF IT.

## S E C T I O N I.

## GENERAL OBSERVATIONS ON THE PRESENT STATE OF ELECTRICITY.

**T**HAT real progress has been made in electricity, has, I presume, been sufficiently demonstrated in the course of the preceding history; that a great deal still remains to be done, will, I think, be evident from this part of the work. Those persons who think that nothing has been done to any purpose in Natural Philosophy, or that the advances have been made very slowly, since the time of Sir Isaac Newton, need only read the preceding history, to be convinced, both that a great deal has been done, and that the progress in this kind of knowledge, instead of being slow, has been amazingly rapid. To quicken the speed of philosophers in pursuing this progress, and at the same time, in some

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measure,

measure, to facilitate it, is the intention of this treatise, and more especially of this part of it. When a traveller imagines he is near his journey's end, he is little solicitous about making dispatch, thinking that, without any haste, the labour of the day will quickly be over ; whereas, if he find that, whatever progress he may have made, he has a great deal still to make, he continues, or quickens his speed.

THE principal reason why many ingenious persons have so soon got to their *ne plus ultra* in philosophical discoveries, has evidently been their attachment to favourite theories ; which they imagined both accounted for all the phenomena that had been observed, and would likewise account for all that should be observed. Having therefore attained to the great object of a science, and discovered the ultimate and most general principles of it, there was nothing more that was worth their notice ; it being beneath men of genius to spend their time in diversifying effects, when there were no new causes to be found. I hope that what has hitherto been said concerning the nature and use of hypotheses, and about the progress and present imperfect state of those which respect electricity, will convince those electricians who may not yet have been convinced of it, that our business is still chiefly with *facts*, and the *analogy of facts* ; that far too few of these have been discovered to ascertain a perfect general theory, and that all that the present hypotheses can do for



us must consist in suggesting farther experiments.

IF we look back upon the history of electricity, and consider the state of facts and of hypotheses at any particular period of time past, we shall see that there was always the same apparent reason for acquiescing in what had been done, as at present. The theories of the first electricians, lame and imperfect as they were, were yet sufficient to account for all the facts they were acquainted with; and as for other facts, they could have no idea or apprehension of them, and therefore could not be solicitous about them.

MR. BOYLE, no doubt, was as fully satisfied with his simple hypothesis of the unctuous effluvia, as Mr. Nollet with his theory of affluences and effluences; or the greatest part of the present race of electricians with that of positive and negative electricity. Mr. Hauksbee, when he made his surprising discoveries concerning the properties of electric light, and many curious circumstances concerning electric attraction and repulsion, might very naturally think that little more was to be done. Indeed, who could have thought otherwise, when the science was actually at a stand for several years after him? All that the indefatigable Mr. Grey (who made the great discovery of the communication of electric powers to bodies not electric per se) imagined to remain undone, were mere chimeras and illusions. Mr. Du Fay, who made the discovery of vitreous and resinous electricity,

had no idea of the electric shock; and the German philosophers, who accidentally observed it, knew nothing of its most remarkable properties. Notwithstanding a great number of treatises on the subject of electricity appeared presently after this discovery, and some of them very systematical, comprehending, no doubt, what the authors of them thought to be the whole of the science, yet none of them had the least idea of the amazing discoveries of Dr. Franklin, relating either to the Leyden phial, or to the nature of lightning. And though numbers of Dr. Franklin's admirers thought that he had exhausted the whole subject, he himself was far from thinking so; and the history of electricity, since the date of his capital discoveries, demonstrates that his suspicion was true.

It may be said, that there is a *ne plus ultra* in every thing, and therefore in electricity. It is true: but what reason is there to think that we have arrived at it. Mr. Grey might have used the same language above twenty years ago; but every body will now acknowledge, that it would have been above twenty years too soon: and yet, I think, it is evident, that Mr. Grey had really more reason to think he had arrived at the *ne plus ultra* of electricity, than we have to think that we are arrived at it. Time has brought to light a great number of *incomplete*, as well as complete experiments, and perhaps more of the former than of the latter; concerning all which, as he could have no knowledge, so  
he

he could have no doubts ; so that, though we know much more than he did ; we, at the same time, know how much more is unknown better than he could. Hitherto the acquisition of electrical knowledge has been like the acquisition of riches : the more we possess, the more we wish to possess ; and, I hope, the more indefatigable we shall be to acquire the possession of it.

ONE thing extremely useful to the progress of farther discoveries, is to know what has really been done by others, and where the science stands at present. For want of this knowledge, many a person has lost his time upon experiments which he might have known had either failed or succeeded with others ; and which it was, therefore, not worth his while to repeat. But the sources of this kind of information are too much scattered, and too distant for most persons to have access to them. This was the first motive of the present undertaking, intended to exhibit a distinct view of all that has been done in electricity to the present time, and likewise the order and manner in which every thing has been done ; that electricians, having a distinct idea of what the progress of electrical knowledge has been, might see more clearly what remains to be done, and what pursuits best promise to reward their labour.

INDEED it is almost impossible for any person to read the history of electricity without gathering many hints for new experiments. When he has the whole before him at one view,

view, he can better bring the distant parts together; and from the comparison of them, new lights may arise. When he sees what experiments have failed, and what have succeeded; what branches of the science have been most attended to, and what things seem to have been overlooked; what has been discovered by accident, and what by theory; when he sees both the true lights which directed some happy discoverers, and the false lights which misled others, he will have the best preparation for pursuing his own inquiries.

To point out many of the *desiderata* in the science of electricity, I am sensible, will, for this reason, be superfluous to many persons, and probably to most who will have read thus far of this treatise: for sufficient hints of them must have been suggested by the perusal of the history. But if I have been anticipated in this part of my work by some of my readers, it will not displease them to find it; and to others, the contents of this chapter will be peculiarly useful.

If, indeed, I had consulted my reputation as a writer, or a philosopher, I should not have attempted this chapter at all. For not only will many of the articles which I shall now put down as *desiderata* in the science be soon no longer so, and even young electricians be able to give satisfactory answers to some difficult queries I am going to propose; but many of them will probably appear idle, frivolous, or extravagant ones; and, in a more advanced

advanced state of the science, it will hardly be imagined why I put them down at all. But if this chapter be a means of hastening so desirable an event, and of accelerating the progress of electrical knowledge, I am very willing that it should, ever after, stand as a monument of my present ignorance.

“THESE thoughts,” to adopt the words of Dr. Franklin, with much more propriety than he himself first used them, “are many of them crude and hasty; and if I were merely ambitious of acquiring some reputation in philosophy, I ought to keep them by me, till corrected and improved by time and farther experience. But since even short hints and imperfect experiments, in any new branch of science, being communicated, have often times a good effect, in exciting the attention of the ingenious to the subject, and so become the occasion of more exact disquisitions, and more complete discoveries; you are at liberty,” says he, to Mr. Collinson, “to communicate this paper to whom you please, it being of more importance that knowledge should increase, than that your friend should be thought an accurate philosopher.”

I would not even propose to draw up the following *queries* upon the plan of those of Sir Isaac Newton, at the end of his treatise on Optics. Many of them are such, that I have hardly the most distant expectation of their being verified; but the attempt to verify them  
may

may possibly lead to some other discoveries of more importance. They are such random thoughts as led to the new experiments I have made; and not having any more leisure to pursue them myself, I freely impart them to my reader, that he may make as much advantage of them as he can: being determined, upon taking leave of the subject, *to write myself fairly out*, as Mr. Addison says; or, as the Spanish writers say, *to leave nothing in my inkhorn*.

HAPPY would it be for science, if all philosophers who are engaged in the same pursuits, would make one common chapter of all their hints and queries: and greatly honoured should I think myself if the present chapter in this treatise might be made use of for that purpose, and if, in future editions of the work, it should be looked into as the common receptacle of the *present desiderata* among the whole body of electricians, and of their imperfect hints for new discoveries. With pleasure should I see each of them distinguished by the name of some generous and illustrious contributor. A few, the reader will find, have been added to my own, and are distinguished in this manner.

MANY persons can throw out hints, who either have not leisure, or a proper apparatus for pursuing them: others have leisure, and a proper apparatus for making experiments, but are content with amusing them-

themselves and their friends in diversifying the old appearances, for want of hints and views for finding new ones. By this means, therefore, every man might make the best use of his abilities for the common good. Some might strike out lights, and others pursue them; and philosophers might not only enjoy the pleasure of reflecting upon their own discoveries, but also upon the share they had contributed to the discoveries of others.

## SECTION

## SECTION II.

QUERIES AND HINTS CALCULATED TO  
PROMOTE FARTHER DISCOVERIES IN ELECTRICITY.

## I.

QUERIES AND HINTS CONCERNING THE  
ELECTRIC FLUID.

**W**HAT is the proportion of the several colours in electric light, in different cases, and in different appearances of it?

Is not the electric light a real-vapour ignited, similar to that of phosphorus; and may not experiments be, hereafter, made, where we shall have the explosion, the shock, and the other effects of electricity, without the light? Is the electric light ever visible except in vacuo? In the open air the electric fluid makes itself a vacuum in order to its passage.

COLLECT the electric fluid, not from the general mass of the earth but from bodies of particular kinds, and observe if it have any different properties, with respect to light, &c.

I

Is



Is it exactly the same at sea, as on land; below the surface of the earth, as above it, &c. &c. &c.?

DR. FRANKLIN observed, that iron was corroded by being exposed to repeated electric sparks. Must not this have been effected by some acid? What other marks are there of an acid in the electric matter? May not its phosphoreal smell be reckoned one?

Is there only one electric fluid, or are there two? Or is there any electric fluid *sui generis* at all, distinct from the ether of Sir Isaac Newton? If there be, in what respect does it differ from the ether?

ARE the particles which affect the organ of smelling, as well as the particles of light, parts of the proper electric fluid, or are they merely adventitious, being, some way or other, brought into action by electricity?

DOES not some particular order of the particles, which Sir Isaac Newton supposes to be continually flying from the surfaces of all bodies, constitute the electric fluid; as others, he imagined, constitute the air, and others the ether, &c.?

Is it probable that there is even any temporary, or growing addition to, or diminution of the whole stock of electricity?

WHENCE arises the elasticity of the electric fluid, and according to what law do its particles repel one another? *Mr. Price.*

Is not the electric matter the same with, or at least, hath it not some near relation to that luminous matter which forms the solar atmo-

atmosphere, and produces the phenomenon called the *zodiacal light*; which is thrown off principally, and to the greatest distance, from the equatorial parts of the sun, in consequence of his rotation on his axis, extending visibly in the form of a luminous pyramid, as far as the orbit of the earth; and which, according to Monsieur De Mairan's ingenious, and at least, plausible hypothesis, falling into the upper regions of our atmosphere, is collected chiefly towards the polar parts of the earth, in consequence of the diurnal revolution, where it forms the aurora borealis?

MAY not the sun be the fountain of the electric fluid; and the zodiacal light, the tails of comets, the aurora borealis, lightning, and artificial electricity as it is various, and not very dissimilar modifications\*?

DID not the sulphureous smell draw our attention towards the vitreous acid, the peculiar hissing noise accompanying the electric blast, spontaneously issuing, for instance, from the pointed wire of a fully charged phial, appears rather to mimic the explosive action of deflagrating nitre; and may, accordingly, without much violence, be supposed to arise from the nitrous aerial acid, violently commensurating with the phlogiston, which it either meets with in the air, or which is conveyed to it by the electrified body. Or, were we to adopt the hypothesis of two distinct electric

\* Monthly Review, October 1767, p. 353.

fluids,

fluids, we might, by way of temperament, propose as a query, whether the nitrous acid, &c. may not be the constant concomitant of those explosive pencils of light, which are observed to dart from the points of bodies replete with the vitreous electricity; while the silent and languid luminous specks (resembling the small tip of a lighted match) appearing on the extremities of bodies endued with a resinous electricity, may as probably indicate the accension of a sulphureous matter, and consequently the presence of the vitreolic acid, the electric spark of explosion, appearing on the approach of the two bodies, being considered as the effect, at least, in part of the menstrual action of these two acids on each other, &c. \*

## II.

### QUERIES AND HINTS CONCERNING ELECTRICS AND CONDUCTORS.

IN what does the difference between electrics and conductors consist? In other words, what is it that makes some bodies permeable to the electric fluid, and others impermeable to it?

ARE the pores of electric bodies smaller than those of conductors, and do they con-

\* Monthly Review, December 1767, p. 458.

tain very much, or very little of the electric fluid?

WHAT is it in the internal structure of bodies that makes them break with a polish? Perhaps all solid electrics do so.

HAS elasticity any connection with electricity, some electrics being extremely elastic?

WHAT is the reason why, in some of Mr. Hauksbee's experiments, the electric light was visible through a considerable thickness of very opaque electrics, as rosin, sulphur, pitch, &c. but not through the thinnest metallic conductors?

WHAT similarity is there in the processes of calcination, vegetation, animalization, and in some measure crystallization; since all bodies which have gone through any of those processes, and perhaps no others, are found to be electrics?

ARE not both electrics and conductors more perfect in their kind in proportion to their specific gravity?

WILL not water conduct electricity the best in its state of greatest condensation; and metals the least in their greatest expansion, as shown by a pyrometer?

COMPARE the invisible effluvia of water with the invisible effluvia of a burning candle, and also those proceeding from other bodies, with respect to their power of conducting electricity.

OBSERVE what degree of heat will discharge any given degree of electricity, in order

der to find in what degree heat makes air a conductor.

### III.

#### QUERIES AND HINTS CONCERNING EXCITATION.

WHAT is the difference, in the internal structure of electrics, that makes some of them excitable by friction, and others by heating and cooling?

WHAT have friction, heating, cooling, and the separation after close contact in common to them all? How do any of them contribute to excitation? And in what manner is one, or the other kind of electricity produced by rubbers and electrics of different surfaces?

Is not Mr. Æpinus's experiments of pressing two flat pieces of glass together, when one of them contracts a positive and the other a negative electricity, similar to the experiments of Mr. Wilcke concerning the production of electricity by the liquefaction of various substances in others; when the substance which melts and contracts is in one state, and that which contains it is in the opposite? And are not both these cases similar to the excitation of the tourmalin, &c. by heating and cooling? In this case may not the tourmalin and the air act upon one another and be in opposite states?

Is not the circumstance common to all these cases, some affection of that space near the surface of the bodies in which the refractive power lies? When bodies which have been pressed together within that space recede from one another, more surface, and consequently more of that space is made, doth not the electric fluid flow into it from that body which has the least power of retaining it, and which it can permeate with the most ease; when not being able to enter the substance of the other, it rests upon its surface?

ARE not the particles of the electric and rubber thrown into a vibration in the act of excitation, which makes frequent recedings of the parts from one another, and thereby promotes the effect above mentioned?

WHAT is the real effect of putting moisture or amalgam upon the rubber? Do not those substances increase the power of excitation, as conductors more distant from the smooth glass, in the gradation of electrics, than the surface of the leather? Or do they only make the rubber touch in more points, or alter the surface of the rubber?

HAS that difference of surface on which colour depends any influence upon the power of excitation?

THE tourmalin and a vessel of charged glass hermetically sealed are both excited by heating and cooling. What other properties have they in common?

## IV.

QUERIES AND HINTS CONCERNING ELECTRIFICATION.

Does electrification increase the exhalation of vapours either from cold or from boiling water? If it do, is the increased exhalation the same in all states of the atmosphere?

Does not the electric matter pass chiefly on the surface of bodies?

Is the action of electrified bodies upon one another more properly an attraction or a repulsion?

Would not continued electrification promote putrefaction?

In what manner is the mutual repulsion of two bodies electrified negatively performed? Is it by the attraction of the denser electric fluid in the neighbourhood, by the quantity of it which may be supposed to be accumulated on the surfaces of such bodies in the manner described p. 430, or to the mutual repulsion of the particles of matter of which the bodies consist?

**V.****QUERIES AND HINTS CONCERNING THE  
POWER OF CHARGING ELECTRICS.**

WHAT is the real operation of conductors in coating electric substances ?

WHAT is the *maximum* of charging a glass jar, with respect to the quantity of its surface, covered by the coating ? It is evident that some jars will discharge themselves, when only a small part at the bottom of them is coated, and when the explosion is very inconsiderable.

ENDEAVOUR to charge a plate of glass with the coating pressed into actual contact with its surface, by means of heavy weights. Also endeavour to excite a plate of glass in the same manner. It is pretty certain that, in the usual method of exciting and charging, the real substance of the glass is not touched ; and though water be attracted by glass ; it may only be to a certain distance from it.

**VI.****QUERIES CONCERNING THE ELECTRICITY  
OF GLASS.**

THROUGH what thickness of glass will an excited electric, of any given strength, attract  
and



and repel light bodies? Is not the same thickness the limit of charging glass with the electric fluid?

Is not a plate of glass contracted in its dimensions by charging, the two electricities strongly compressing it, so as to increase its specific gravity?

Is the tone of a glass vessel, made in the form of a bell, the same when it is charged as when it is uncharged? Or would the ringing of it make it more liable to break in those circumstances?

Does the electric fluid with which glass is charged reside in the pores of the glass, or only on its surface; or rather within the space that is occupied by the power of refraction, i. e. a small space within, and likewise without the surface?

Is the refractive power of glass the same when it is charged or excited?

How does the different refractive power of glass, or its density (which is probably in the same proportion with its refractive power) affect its property of being excited or charged?

Is there not a considerable difference in glass when it is new made, and when it has been kept a month or two, both with respect to excitation and charging?

LET glass of every different composition be tried both with respect to excitation and charging. Would it not be found that differences with respect to metallic ingredients,

hardness, annealing, continuance in fusion, &c. would influence both the properties ; and that, in several cases, the same circumstance that was favourable to one would be unfavourable to the other ?

GLASS has hitherto been supposed to be full of the electric fluid, and its impermeability has been accounted for upon the difficulty with which the electric fluid moves in its pores. But may we not suppose the substance of glass to be absolutely impermeable to electricity, that no foreign electric matter ever so much as enters a single pore of it, but lodges wholly on its surface ; for instance, between the point of contact and the real surface, or within the limits of the refractive power ; that is, a little way on both sides the surface. This place is, I think, on many accounts, extremely convenient to dispose of the electric matter, whether we make it to consist of two fluids, or of one. Their being kept asunder, if there be two, or its being prevented from getting through, if there be but one, will be much easier to conceive in this case, than upon the supposition that the electric fluid *can* enter and move in the substance of the glass, though it can only enter and move with difficulty, as Mr. Æpinus expresses it. For, let the motion be ever so difficult, one would think that this circumstance could only make it move so much the slower, and that, give the electricity in the charged plate of glass time enough, and it would at length, without any  
exter-

external communication, perform the journey to the other side, whither it has so strong a tendency to go.

MOREOVER, one would think, that, upon the hypothesis of the admission of the electric fluid within the pores of the glass, when the discharge of a phial was actually made through the substance of the glass, it might be in a silent manner, without breaking the glass; whereas when the surfaces of the glass are supposed to be violently pressed, and the pores of it not in the least entered by any particle of the fluid, or fluids, the impossibility of the electric charge getting through the glass is evident, as well as the necessity of its breaking the glass, if it do force a passage.

## VII.

### QUERIES AND HINTS CONCERNING THE EFFECT OF ELECTRICITY ON ANIMAL BODIES.

Is the fluid on which electricity depends, at all concerned in any of the functions of an animal body? In what manner is the pulse of a person electrified quickened, and his perspiration increased?

MAY not the increased perspiration of an animal body be greater in a moist atmosphere than in a dry one, there being then more conducting particles in the atmosphere, to act and react upon the effluvia in the pores of the body;

body; on which the copious perspiration does, probably, in a great measure, depend?

## VIII.

QUERIES AND HINTS CONCERNING THE  
ELECTRICITY OF THE ATMOSPHERE.

IN what manner do the clouds become possessed of electricity?

Does the wind in any measure contribute to it?

Is it effected by the gradual heating and cooling of the air? If so, whether is it the heating or the cooling that produces positive electricity? Which ever it be, the contrary will probably produce negative electricity. Let the experiment be made by an electrical kite. *Mr. Canton.*

As thunder generally happens in a sultry state of the air, when it seems replenished with some sulphureous vapours; may not the electric matter then in the clouds be generated by the fermentation of sulphureous vapours with mineral or acid vapours in the air? *Mr. Price.*

MR. WILCKE supposes the air to contract its electricity, in the same manner as sulphur and other substances do, when they are heated and cooled in contact with various bodies. Thus the air, being heated or cooled in the neighbourhood of the earth, gives electricity to the earth, or takes it from it, and the electrified

trified air, being conveyed upwards, by various means, communicates its electricity to the clouds \*.

LET rain, snow, and hail be received in insulated vessels, in different states of the atmosphere, to observe whether they contain any electricity, and in what degree.

MAY not the void space above the clouds be always occupied with an electricity opposite to that of the earth? And may not thunder, earthquakes, &c. be occasioned by the rushing of the electric fluid between them, whenever the redundancy in either is excessive? Is not the aurora borealis, and other electrical meteors, which are remarkably bright and frequent before earthquakes, some evidence of this?

Is not the earth in a constant state of moderate electrification, and is not this the cause of vegetation, exhalation, and other the most important processes in nature? These are promoted by increased electrification. And is it not probable that earthquakes, hurricanes, &c. as well as lightning, are the consequence of a too powerful electricity in the earth?

SUPPOSING earthquakes to be caused by the discharge of a redundant electricity from the surface of the earth, might they not be prevented, in countries subject to them, by kites constantly flying very high, with wires in the strings, so as to promote an easy communication between the earth and the upper regions of the atmosphere?

\* Remarks on Franklin's Letters, p. 302.

## SECTION III.

BRANCHES OF KNOWLEDGE PECULIARLY  
USEFUL TO AN ELECTRICIAN.

**I**N the historical part of this work I have shown what has been done on the subject of electricity, and under the preceding *defiderata*, I have endeavoured to give some idea of what yet remains to be done, with a few hints concerning farther experiments. In the close of this part, I would willingly do something more towards enabling my reader to make farther advances in electrical inquiries. However, all that can be done in this way must, in its own nature, be more imperfect than even the account of the *defiderata*: for it is evident, that he who is able to teach others to make discoveries might make them himself. Notwithstanding this, it is possible that some general observations may be of use to this purpose; such for instance as Lord Bacon makes, in his *Novum Organon*; a book which, though it contain few or no philosophical discoveries itself, has contributed not a little to the discoveries contained in others. A few such general observations, confined to the subject of electricity, I shall endeavour to suggest in this place.

It is an observation which the progress of science daily confirms, that all truths are not  
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only consistent, but also connected with one another. The observation has, with no small appearance of justice, been extended even to the arts; there being no two of them so remote, but that some of the methods and processes used in the one have some analogy to some that are used in the other. Hence the knowledge of one art or science is subservient to the knowledge of others; and no person can presume that he is perfectly master of any one, till he has received all the assistance he can from, at least, all its sister arts or sciences.

INDEED the very existence of the various arts and sciences is almost a demonstration of their relation to each other. For it were highly unreasonable to suppose, that the elements of any new art or science were discovered by means independent of the study or practice of those already known. As it is by easy transitions that we pass from one part of any particular science to another, so it is by transitions equally easy that mankind have passed from one distinct science to another. Consequently, to those previously discovered arts and sciences must we have recourse, in order to understand the full evidence, on which the first principles of any new art or science rest.

ELECTRICITY is by no means an exception to this general rule. It has its sister sciences as well as others. In pursuance of them were its own principles first discovered; and the farther prosecution of electrical experiments has

has shown its connection with more sciences than it was at first apprehended to have any relation to. Now the study of all these cannot but, reciprocally, contribute to perfect and extend the knowledge of electricity.

GILBERT, the first of modern electricians, was led to make his electrical experiments by their relation to those of magnetism, into which he was professedly inquiring. The study of chymistry seems to have led Mr. Boyle to attend to electricity, as well as to other occult qualities of particular bodies. Electric light was considered by all those who first observed it as a species of phosphorus; and with this view was Mr. Hauksbee conducted in all the experiments he made upon it.

THESE, and other discoveries in electricity, having been made thus indirectly, excited the attention of philosophers to the subject, and induced them to sit down to the study of it in a direct and professed manner. Upon this it soon appeared, that electricity was no secondary, or occasional, but a principal, and constant agent in the works of nature, even in some of its grandest scenes; and that its agency, far from being confined to bodies of a particular class, extended its influence to all without exception; that the mineral, vegetable, and animal world, with the human frame in a particular manner, were all subject to its power; and that electrical experiments and principles enter into the most interesting arts and sciences which have them for their object.



object. We also see every day, that electricity is extending itself still more into the subjects of other sciences, both by means of the analogy of their operations, and also by their reciprocal influences.

ON these accounts, to be an electrician at present, requires a much more extensive fund of various knowledge than it did but ten years ago; and a man must have a very comprehensive knowledge of nature in all its known operations, before he can reasonably expect to make any farther discoveries. For it can only be by applying electricity to various parts of nature, and by combining its operations with other operations, both of nature and art, that any thing new can be found out. Almost all that can be done by the common electrical machines, and the usual apparatus of them, has been done already; so that we must look farther in quest of new discoveries. I hope, therefore, that I shall be excused, if I endeavour to give a hint of that kind of knowledge which, I apprehend, may be peculiarly subservient to improvements in electricity, and furnish views and materials for new experiments.

NATURAL PHILOSOPHY cannot but be of the greatest use for this purpose; but, of all its branches, none promises to be of more use to the electrician than CHYMISTRY. Here seems to be the great field for the extension of electrical knowledge: for chymistry and electricity are both conversant about the latent and less obvious properties of bodies; and  
yet

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yet their relation to each other has been but little considered, and their operations hardly ever combined; few of our modern electricians having been either speculative or practical chymists.

AMONG other branches of Natural Philosophy, let the doctrine of LIGHT AND COLOURS be also particularly attended to. It was this that Newton thought would be the key to other, at present, occult properties of bodies.

LET particular attention be also given to every thing that the imperfect state of Natural Philosophy furnishes respecting the ATMOSPHERE, its composition and affections. The phenomena of lightning show the connection of this subject with electricity; and, probably, electricity may be our key to a much more extensive knowledge of meteorology than we are yet possessed of.

THE shock of the Leyden phial, the discovery of the sameness of lightning and electricity, together with the cure of several diseases by electrical operations, are sufficient to convince us of the singular importance of the study of ANATOMY, and every thing relating to the animal œconomy to an electrician. And had physicians more generally attended to electricity, as an article of the materia medica, many more important and useful discoveries might, no doubt, have been made. Enough, however, have been made to excite us to farther inquiries.

MR.

MR. ÆPINUS has lately given us an excellent specimen of what use MATHEMATICS, and especially algebraical calculations, may be to an electrician; and their use will probably, in time, be found still more extensive.

As electricity has much to expect from several branches of Natural Philosophy, so it will be ready, in its turn, to lend its assistance to them. It already supplies arguments and proofs of some principles in Natural Philosophy, which strengthen those that are drawn from other quarters. By electricity, as well as by the principles of light and colours, we can demonstrate, that it requires a considerable force to bring bodies which are contiguous to one another, and even lie upon one another, into actual contact; and the moisture of the air may perhaps be shown to more exactness by Mr. Canton's electrical balls than by any other hygrometer whatever. But I do not mean to pursue this subject, and only mention these cases by way of example.

UPON the subject of the proper furniture for an electrician, I think it may be justly added, that a knowledge of MECHANICS will be useful to him; by which I mean, upon this occasion, not only the theory, but in some measure the practice too. For without some mechanical knowledge of his own his electrical machinery will be very often out of order, and but ill answer his purpose.

IF, indeed, a person mean nothing more than to amuse himself and his friends with the experiments that have been made by others (and this is a method of amusement which I am far from discouraging) the machines he may purchase, ready constructed to his hands, will answer his purpose very well; and the directions which are usually given along with the machines will enable him to perform the common experiments with tolerable certainty: and if any damage should happen to his apparatus, a mathematical instrument-maker (if he happen to live in or near a large town) can readily repair it for him. But if a man propose to study the subject of electricity as a philosopher, with a view to extend the knowledge of it, the assistance of others will not be sufficient for him.

THE common electrical machines, and the usual electrical apparatus, will enable a person to do little more than exhibit the common experiments. If he propose to go farther, he must diversify his apparatus; he must often alter the construction of his machines, and will find that common workmen cannot execute any thing out of their usual way, without more than general directions. Besides, unless a person be fortunately situated, workmen of every kind cannot always be at hand, to do every little thing he may want in the mechanical way, whenever he may happen to get a hint of a new experiment that requires it.

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AN electrician, therefore, ought never to be without the common tools of a *cabinet-maker, clock, and watch-maker*, at least, and know, in some measure, how to use them. With respect to glass, he ought, by all means, to learn the use of a *blow-pipe*, the method of drawing out and bending glass tubes, and performing, with some degree of dexterity, other operations upon glass, which he will want to use in a great variety of forms. An electrician, thus furnished, will be able, upon any occasion, to serve himself: and the slowness and blunders of mechanics do but ill suit with the ardour of persons engaged in philosophical inquiries.

It were much to be wished, that philosophers would attend more than they do to the construction of their own machines. We might then expect to see some real and capital improvements in them; whereas little can be expected from mere mathematical instrument makers; who are seldom men of any science, and whose sole aim is to make their goods elegant and portable.

FORMERLY, indeed, philosophers were obliged to construct their own machines. Mr. Boyle, Mr. Hawksbee, and Dr. Desaguliers would have done nothing by giving tradesmen orders for what they wanted. There were no such things to had. Necessity therefore drove them to the study

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and practice of mechanics, and from their contrivances are derived almost all the philosophical instruments which are now in use.

EVERY original genius, like them, must, in this respect, follow their steps. He will extend his views beyond the power of the present machinery, which can only be adapted to the present state of science. And, I think, one principal reason of the imperfect state of several branches of electrical knowledge with us, may be evidently traced to some general imperfections in the structure of all our common machines in England; which render several kinds of experiments very difficult, or almost impossible to be made; as may be shown in the next part of the work, in which I shall treat at large of the construction of machines, and give the best directions I am able for using them.

LASTLY, if an electrician intend that the public should be benefited by his labours, he should, by all means, qualify himself to draw according to the rules of PERSPECTIVE; without which he will often be unable to give an adequate idea of his experiments to others. There is so much beauty in the rules of this ingenious art, and so much pleasure in the application of them, that I cannot help wondering, that all gentlemen of a liberal education do not take the small degree of pains, that is necessary to make them-

themselves masters of it. All the mechanical methods of drawing, especially where a great number of right lines are used, as in drawing machines, &c. are exceedingly imperfect, and insufficient. They admit not of half the variety of perspective drawings. They can hardly ever be near so correct; besides that, I know by experience, they take up much more time, and the operation is exceedingly slavish and troublesome.

## P A R T V.

OF THE CONSTRUCTION OF ELECTRICAL  
MACHINES, AND THE PRINCIPAL  
PARTS OF AN ELECTRICAL APPA-  
RATUS.

## S E C T I O N I.

GENERAL OBSERVATION ON THE CON-  
STRUCTION OF AN ELECTRICAL APPA-  
RATUS.

**I**MPROVEMENTS in electrical machines have, as might well be expected, kept pace with improvements in the science of electricity. While nothing more than electrical attraction and repulsion were known, nothing that we should now call an *electrical apparatus* was necessary. Every thing that was known might be exhibited by means of a piece of amber, sealing-wax, or glass; which the philosopher rubbed against his coat, and presented to bits of paper, feathers, and other light bodies that came in his way, and cost him nothing.

To give a greater degree of friction to electric substances Otto Guericke and Mr. Hauksbee



Hauksbee contrived to whirl sulphur and glass in a spherical form; but their limited knowledge of electricity did not suggest, or require the more complex structure of a modern electrical machine: Mr. Hauksbee's contrivances, indeed, were excellent, and the apparatus for many of his experiments well adapted to the purposes for which they were intended.

WHEN no farther use could be made of globes, philosophers had recourse to the easier and cheaper apparatus of glass tubes, and sticks of sulphur or sealing-wax; and the first conductors they made use of were nothing more than hempen cords supported by silken lines. To these, bars of metal were soon substituted. After that, recourse was again had to the globe, as much more convenient to give an uniform supply of electric matter to these insulated conductors; and, in due time, a rubber was used to supply the place of a human hand.

THE discovery of the Leyden phial occasioned still more additions to our electrical apparatus; and the more modern discoveries of Dr. Franklin and others have likewise made proportionable additions highly requisite. No philosopher, for instance, can now be satisfied, if he be not able to supply a conductor from the clouds, as well as from the friction of his glass globes or tubes. But having already marked the progress of improvements in electrical machines, as well as in electrical science, I shall content myself with this brief

recapitulation, and proceed to describe what experience (in many cases dear bought) has taught me to think the best method of constructing machines, and to lay down the best rules for conducting electrical operations.

NOTWITHSTANDING globes or cylinders are now of the most extensive use in electrical experiments, GLASS TUBES are, nevertheless, most convenient for several purposes, and no electrician ought to be without them. They should be made as long as a person can well draw through his hand at one stroke, which is about three feet, or something more; and as wide as he can conveniently grasp. The thickness of the metal is not material, perhaps the thinner they are, the better, if they will bear sufficient friction; which, however, needs only to be very gentle, when the tube is in good order. It is most convenient to have the tube closed at one end: for, besides that the electric matter is thereby retained best on its surface, the air may more easily be drawn out of it, or condensed in it, by means of a brass cap fitted to the open end. A tube thus furnished is requisite for various experiments. [*a. Pl. II.*]

THE best rubber that has yet been found for a smooth glass tube is the rough side of black oiled silk, especially when a little amalgam of mercury and any metal, is put upon it.

AN electrician should be furnished with rough glass tubes, i. e. tubes with their polish taken

taken off, as well as with smooth ones; but a cylinder of baked wood will do nearly as well. The best rubber for a rough glass tube, or a cylinder of baked wood, as well as for a stick of sulphur or sealing-wax, is soft new flannel; or rather skins, such as hare skins, or cat skins, tanned with the hair on, being smoother, and having a more exquisite polish.

ELECTRICIANS are not quite agreed whether the preference is, upon the whole, to be given to GLOBES or CYLINDERS. In favour of cylinders it is said, that more of their surface may be touched by the rubber. On the other hand, in favour of globes, it is said, that they can more easily be blown true, so as to press the rubber equally; they may also be made larger in diameter, and by this means, the axis (if they have any) may be farther from the excited surface: for when the *axes* are near the surface, the electric fire will seem to strike them, so that they will sometimes appear luminous in the dark, and if they be insulated, the extremities of the axes will give sparks; which is certainly a diminution of the electric fire at the conductor.

FOR this reason, I would advise, that all *axes* be avoided as much as possible, having found by experience, that they are in no case whatever necessary, the largest globes being whirled horizontally, with the greatest ease, and in every respect to more advantage, with one neck than two. This method of fitting  
up

up globes also makes electrical machines much less complex, expensive, and troublesome.

LET every globe intended to be thus fitted up have its neck inclosed in a pretty deep brass cap, ending in a dilated brim, of about half an inch broad, if the globe be a large one. To this neck let there be fitted a short iron axis, and on that a PULLEY; and let a space of about three quarters of an inch of the axis be left between the pulley and the cap. In this place the axis is to be supported by a strong BRASS ARM [*c.* Pl. VII.] proceeding from the pillar into which the extremity of the axis is put, and in which it turns. This brass arm may be made to receive globes of any size whatever, room being left in it for pulleys of any size that may be wanted for them.

IN this manner globes may be fixed much more truly than they can with two necks, and they are mounted with much more ease, and less expence. The weight of large globes is no objection to this method. The largest need not to weigh above eight or ten pounds, and these have been found to turn with great ease in this manner. The rubber, if it be placed under the globe, will contribute to support the weight of it.

LET there be a hole made in the brass cap above mentioned, in order to preserve a communication between the external air, and the air within the globe: for if the air within the globe be either rarer or denser than that without, the excitation is found to be lessened in  
propor-

proportion; and, judging from experience, nothing is to be apprehended from any moisture which might be supposed to insinuate itself into the globe by such a communication. A difference, however, might be found in a damp situation.

It will be found convenient to have the axis project about an inch beyond the pillar in which it turns, [as at *d*. Pl. VII.] that a handle may be fitted to it, and that it may thereby be turned without a wheel, for the greater variety of experiments.

If an axis be used, let both the extremities of it be carefully turned in a lathe; otherwise it will not turn without a very disagreeable rattling; and let the part within the globe be made round, and smooth, or covered with some electric substance, to prevent its taking off much of the electric virtue of the globe.

ONE of the pillars, in which these globes or cylinders with two axes are turned, should be moveable; for then a globe or cylinder of any size may be used, and they should be made high enough, and have holes at small distances quite to the top, to take two globes upon occasion, one above the other. [Plate VII.]

It has not yet been determined by electricians what kind of glass is the fittest for electrical purposes, but the best flint is commonly used. I have not made so many experiments, as I could wish, to ascertain this circumstance; as they are both very uncertain

tain and expensive; but I have some reason to think that common bottle metal is fittest for the purpose of excitation; at least, the best globe I have yet seen is one that I have of that metal. Its virtue is certainly exceeding great, and I attribute it in part to the great hardness of the metal, and in part to its exquisite polish. The blowing of any thing spherical in this metal, and especially the making large globes smooth is very precarious; and they can hardly be made with two necks.

THE globe above mentioned is about ten inches in diameter, but nothing has been determined about the best size. I have used almost every size, from three inches to near eighteen in diameter, without knowing what advice to give. Perhaps *ceteris paribus*, twelve or thirteen inches may be, upon the whole, as convenient as any; but much larger, if they could be whirled with the same ease, would probably do better.

IF a person chuse to have no assistant, but would turn the globe, and manage the apparatus himself, (which is, on many accounts, very desirable) it will be most convenient to have the axis of the WHEEL level with the table at which he sits. But if he chuse rather to stand all the time he is making his experiments, it should be raised proportionably higher. It will, perhaps, be most convenient to make the diameter of the wheel about eighteen inches; and the diameter of the pullies should be such as will give them, at least, four or five revolutions for one of the wheel.

wheel. For the globe should generally revolve at least four or five times in a second, which is much swifter than it can well be turned without a wheel. But if a globe be very large, a wheel is less necessary.

THE wheel should be made moveable with respect to the frame in which the globes are hung, or the frame should be moveable with respect to the wheel, to suit the alterations which the weather will make in the length of the *string*, particularly if it be made of hemp; but worsted makes an excellent string, and is not so apt to alter with the weather, and a leathern strap is perhaps better than either. If the distance between the wheel and the pulley cannot be altered, the operator must occasionally moisten his hempen string, in order to make it tighter, which is, on many accounts, very inconvenient. Several grooves in the same wheel are very useful, and almost necessary, if more than one globe be used at the same time. If a flat leathern strap be not used they should be cut sharp at the bottom; as should also the grooves in the pulleys, that the string may lay faster hold of them, and that strings of different sizes may be used.

THE best RUBBERS for globes or cylinders are made of red basil skins, particularly the neck part of them, where the grain is more open, and the surface rather rough. That the rubber may press the globe equally, it should be put upon a plate of metal bent to the shape of the globe, and be stuffed with

any thing that is pretty soft. Bran is good ; and if the stuffing be a conductor, as flax, it will be better than if it be a non-conductor, as hair, or wool. It should rest upon a spring, to favour any inequality there may be in the form of the globe or cylinder. The best position of the rubber, for a variety of purposes, is an horizontal one, but it should be capable of being placed in every variety of horizontal position ; and the spring which supports the rubber should be made to press more or less at pleasure. The rubber should be made nearly as large one way as the other. If it be made very narrow, some parts of the globe will pass it without a sufficient friction. To remedy that inconvenience, the hand (if it be dry) may be held to the globe, just before the rubber, to add to its breadth ; but that posture is very inconvenient.

It is advisable that there be no sharp edges or angles about the rubber, for that would make the *insulation* of it (which is a matter of great consequence) ineffectual. By the insulation of the rubber every electrical experiment may be performed with the twofold variety of positive and negative, and a conductor be made to give or take fire at pleasure. This insulation is best made by a plate of glass, five or six inches in diameter, [*g.* Pl VII.] interposed between the metallic part of the rubber and the steel spring that supports it. When positive electricity is intended to be produced, a chain [*n.* Pl. VII.] must connect the rubber with the floor ; but when negative elec-



electricity is wanted, the chain must be removed, and hung upon the common conductor, while another prime conductor must be connected with the rubber ; which will therefore be electrified negatively.

THE best method of collecting the electric fire from the globe seems to be by three or four pointed wires, [*m. Pl. VII.*] two or three inches long, hanging lightly upon the globe ; and neither so light as to be thrown off the globe by electrical repulsion (which would occasion a loss of the electric matter) nor so heavy as to prevent their separating to a proper distance, and being drawn backwards or forwards, as the most effectual discharge of the fire, accumulated on different parts of the globe, may require. For this purpose they are best suspended on an open metallic ring. Needles with fine points do admirably well.

It is requisite, for a variety of uses, that the PRIME CONDUCTOR be fixed very steady. It ought not, therefore, to hang in silken strings, but have a solid support. In a dry situation baked wood answers very well ; but a hollow pillar of glass lined with sealing-wax is better, as it doth not require so much attention.

For common purposes a small conductor is most convenient, but where a strong spark is wanted, it is proper to have a large conductor at hand, which may occasionally be placed in contact with the smaller, and be removed from it at pleasure. But whatever be the size of a  
prime

prime conductor, the extremity of it, or that part which is most remote from the globe, should be made much larger and rounder than the rest : [*k. Pl. VII.*] for the effort of the electric matter to fly off is always the greatest at the greatest distance from the globe. But for the same reason, if a *long* spark be wanted, the large conductor should terminate in a smaller knob, or an obtuse edge, at which the sparks should be solicited. Experience only can instruct a person what size of a knob, or what thickness of an edge is the best for the purpose. In this respect, the effects are often very different in the same apparatus, when the difference in the circumstances is imperceptible.

As the electrician will have frequent occasion to insulate various bodies, I would advise that he make all the stands and stools which he uses for that purpose of glass tubes lined with sealing-wax, though in a dry situation ; baked wood, especially when covered with a slight varnish, will do very well.

THE electrician, having thus constructed his machine, will want METALLIC RODS, [*s. Pl. II.*] to take sparks from his conductor for various uses. These should have knobs, larger or smaller in proportion to the curvature of the conductor. If the knob be too small, it will not discharge the conductor at once, but by degrees, and with a less sensible effect ; whereas the spark between broad surfaces is thick and strong.

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THE more formidable part of an electrical apparatus consists in the COATED GLASS, that is used for the Leyden experiment. The form of the plate is immaterial with respect to the shock; and, for different experiments, both plates of glass, and jars, of various forms and sizes, must be used. For common uses, the most commodious form is that of a jar, as wide as a person can conveniently hold by grasping, and as tall as it will stand without any danger of falling; perhaps about three inches and a half in diameter, and eight inches in height. The mouth should be pretty open, that it may be the more conveniently coated on the inside, as well as the outside, with tinfoil: but it will generally be most convenient to have the mouth narrower than the belly; for then it may be more easily kept clean and dry, and the cork, when one is wanted, will be easier to manage. But no electrician would chuse to be without a great number of jars of various sizes and forms. A considerable variety may be seen in plate II. fig. *c, d, e, f, g, h, i, j, k*. The form of a coated plate of glass is represented at *b*, in the same plate.

THE method of coating is much preferable to that of putting water or brass shavings into the jars, which both makes them very heavy, and likewise incapable of being inverted, which is requisite in many experiments. Brass dust, however, or leaden shot is very convenient for small phials. These serve very well where it is necessary to remove the coating as soon as

the jar is charged, but, for this purpose, quicksilver will generally answer the best. The tinfoil may be put on either with paste, gum water, or bees wax. To coat the insides of vessels, which have narrow mouths, moisten the inside with gum water, and then pour some brass dust upon it. Enough will stick on to make an exceedingly good coating; and if nothing very hard rub against it, it will not easily come off. This brass dust, which is extremely useful in a great variety of electrical experiments, may be had at the pin-makers.

IN the construction of an ELECTRICAL BATTERY I would not, in general, recommend very large jars. A number of smaller are preferable on several accounts. If one of these break by an explosion, or be cracked by any accident, the loss is less considerable; besides, by means of narrow jars, a greater force (that is a greater quantity of coated surface) may be contained in less room; and, as narrow jars may be made thinner, they will be capable of being charged higher in proportion to their surface than large jars, which must necessarily be made thick. The largest jars that the glass-men can conveniently make are about seventeen inches in height; and they should not be more than three in diameter, and of the same width throughout. Thus they may be easily coated both within and without, and a box of a moderate size will contain a prodigious force: for the jars being coated within two inches of the top, they

they will contain a square foot of coated glass a piece.

THE first battery that I constructed for my own use, consisted of forty-one jars of this size ; but a great number of them bursting by spontaneous discharges, I constructed another, which I much prefer to it, and of which a drawing is given Plate III. It consists of sixty-four jars, each ten inches long, and two inches and a half in diameter, coated within an inch and a half of the top. The coated part of each is half a square foot ; so that the whole battery contains thirty-two square feet. The wire of each jar has a piece of very small wire twisted about the lower end of it, to touch the inside coating in several places ; and it is put through a pretty large piece of cork, within the jar, to prevent any part of it touching the side, which would tend to promote a spontaneous discharge. Each wire is turned round, so as to make a hole at the upper end ; and through these holes a pretty thick brass rod with knobs is put, one rod serving for one row of the jars.

THE communication between these rods is made by laying over them all a thick chain, which is not drawn in the plate, lest the figure should appear too confused. If I chuse to use only part of the battery, I lay the chain over as many rods as I want rows of jars. The bottom of the box, in which all the jars stand, is covered with a plate of tin, and a *bent wire*, touching this plate, is put through the

box, and appears on the outside, as in the plate. To this wire is fastened whatever is intended to communicate with the outside of the battery, as the piece of small wire in the figure, and the discharge is made by bringing the brass knob to any of the knobs of the battery.

THIS is the battery which I have generally used in the experiments related in the last part of this work; though, when I have wanted a very great force, I have joined both the batteries, and even several large jars to them. And it will perhaps be allowed to be some evidence of the goodness of this construction, that after using it so much, I see no reason to wish the least alteration in any part of it. Were I to construct another battery, I should take jars of the very same size, and dispose of them in the very same manner.

To discover the kind and degree of electricity, many forms of ELECTROMETERS have been thought of, as the reader may have perceived in the course of the history; but this business is still imperfect. Mr. Canton's balls are of excellent use both to discover small degrees of electricity, to observe the changes of it from positive to negative, and *vice versa*; and to estimate the force of a shock before the discharge, so that the operator shall always be able to tell, very nearly, how high he has charged his jars, and what the explosion will be whenever he chuses to make it.

MR.

MR. CANTON'S BALLS (represented on a glass standing on the stool [*c* Plate II.] are only two pieces of cork, or pith of elder, nicely turned in a lathe, to about the size of a small pea, and suspended on fine linen threads. It is convenient to have these in small boxes for the pocket; the box being the full length of the strings, that they may lie there without being bent.

MR. KINNERSLEY'S electrometer, described vol. i. p. 255, is useful to ascertain how great shocks have been, and for many curious experiments in electricity. A drawing is given of it [*r*, Plate II.] but the glass tube is represented as much shorter than it was made by Mr. Kinnerley. I think it in general more convenient; as the bore of the small tube may easily be proportioned to it. But if a person get one long tube, of the same size throughout, it may be cut into different lengths, and the same brass caps will fit any part of it.

AT the top of the stand of baked wood which supports Mr. Kinnerley's electrometer, I have fixed another, contrived by Mr. Lane, to give a number of shocks, all of precisely the same degree of strength. It consists of a brass rod furnished with a knob, which, by means of a fine screw, may be fixed at any distance from the prime conductor, or any other fixed body communicating with the inside of a jar or battery. In consequence of this, the jar or battery, with which it is connected, can be charged no higher than the

distance at which those fixed bodies are placed will permit; for at that height of the charge, the explosion will always be made between them. See Mr. Lane's more particular description of this useful instrument\*.

To the account of these articles of an electrical apparatus, which must be used within doors, it will not be wholly insignificant to add, that a strong firm table is highly requisite. For if the TABLE on which the apparatus is disposed be apt to shake, a great number of experiments cannot be performed to advantage.

IN order to repeat the noble experiment of the sameness of the electric fluid with the matter of lightning, and to make farther observations on the electricity of the atmosphere, the electrician must be provided with a MACHINE FOR DRAWING ELECTRICITY FROM THE CLOUDS. For the best construction of such a machine, take the following directions. On the top of any building (which will be the more convenient if it stand upon an eminence) erect a pole [*a* fig. 2. Pl. I.] as tall as a man can well manage, having on the top of it a solid piece of glass or baked wood, a foot in length. Let this be covered with a tin or copper vessel [*b*] in the form of a funnel, to prevent its ever being wetted. Above this let there rise a long slender rod [*c*] terminating in a pointed wire, and having a

\* Phil. Trans. Vol. lvii. p. 451.



small wire twisted round its whole length, to conduct the electricity the better to the funnel. From the funnel make a wire [*d*] descend along the building, about a foot distance from it, and be conducted through an open sash, into any room which shall be most convenient for managing the experiments. In this room let a proper conductor be insulated, and connected with the wire coming in at the window. This wire and conductor, being completely insulated, will be electrified whenever there is a considerable quantity of electricity in the air; and notice will be given when it is properly charged, either by Mr. Canton's balls, hung to it, or by such a set of bells as will be described hereafter.

To make these experiments in perfect safety, the electrified wire should be brought within a few inches of a conducting rod, that serves to guard the house, that the redundant electricity may pass off that way, without striking any person that may happen to stand near it. The conductor to guard the house should consist of one rod, between one fourth and one half of an inch thick, if it be of iron, but smaller if it be brass or copper, terminating upwards in a sharp point, about four or five feet above the highest part of the building; and below it should, if possible, be continued to some well or running water. Otherwise it should be sunk several feet into the ground, at the distance of some yards from the building. It is of no consequence whether this conducting rod be fastened on

the inside, or outside of the house, or how many bendings are made in it.

IF the electrician be desirous of making experiments upon the electricity of the atmosphere to greater exactness, he must raise a kite, by means of a string in which a small wire is twisted. The extremity of this line must be silk, and the wire must terminate in some metallic conductor, of such a form as shall be thought most convenient. Mr. Romas's experiment will perhaps convince my reader, that it may be dangerous to raise this kite at the approach of a thunder storm; and upon this occasion the common apparatus above described for drawing electricity from the clouds will, probably, answer this purpose well enough.

BUT, with the following apparatus, I should apprehend no great danger in any thunder storm. Let the string of the kite [*a* fig. 3. Pl. I.] be wound upon a reel [*b*] going through a slit in a flat board, fastened at the top of it; by which more or less of the string may be let out at pleasure. Let the reel be fixed to the top of a tin or copper funnel [*c*] such as was described above; and from the funnel let a metallic rod [*d*] with a large knob be projected, to serve for a conductor. This funnel and reel must be supported by a staff [*e*] the upper end of which, at least, must be well baked; and the lower end may be made sharp, to thrust into the ground, when the kite is well raised.

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THE safety of this apparatus depends upon the chain [ / ] fastened to the staff, by a hook a little below the funnel, and dragging on the ground: for the redundant lightning will strike from the funnel to the chain, and so be conducted as far as any one chuses, without touching the person who holds the staff.

SPARKS may be taken from the conductor belonging to this apparatus with all safety, by means of a small rod of baked wood [*a* fig. 4.] furnished with a small funnel [*b*] and a brass rod [*c*] and a chain connected with: for the lightning which strikes the rod, will pass by the funnel and the chain, without touching the person who holds the rod.

MR. HARTMAN, in the construction of his apparatus for observing the electricity of the atmosphere with safety, makes use of long silken strings to support his metallic rod. These, therefore, require a large shed, fastened to the rod above them, to keep them dry. And, lest the rain that falls upon this shed should carry off all the electricity, he makes a channel all round, which receives the rain; and thence he conveys it, under the shed, into an insulated receptacle\*. But I cannot help thinking this complex apparatus unnecessary, especially if a solid stick of glass and a small cover be used, instead of the silken strings and large shed.

\* Anmerkungen, &c. p. 38.

## SECTION II.

## A DESCRIPTION OF SOME PARTICULAR ELECTRICAL MACHINES, WITH OBSERVATIONS ON THEIR PRINCIPAL ADVANTAGES AND DEFECTS.

**A**FTER this general account of the construction of electrical machines, and the principal parts of an electrical apparatus, my reader may perhaps expect a more particular account of some of the principal varieties with which they are usually made. And though it may be presumed, from what has been advanced upon that head, that any person might judge for himself, I shall endeavour to gratify those who are willing to provide themselves with an electrical machine, by giving drawings and descriptions of some of the best constructions that have fallen under my notice, observing what I apprehend to be their several advantages and defects.

I SHALL begin with Mr. Hauksbee's machine [Plate IV. fig. 1.] which is an excellent construction considering the state of electricity in his time. The drawings annexed will render a very particular description of this, or the other machine, unnecessary. This has no rubber, no prime conductor, or field for making experiments; for no such things were wanted in his time: but it may be easily accommodated

modated with them all. A conductor may hang from the cieling, a rubber may be supported by a spring fixed under the globe, and a table placed near the machine, may receive the apparatus necessary for making experiments. The inconveniencies of this construction are, that the operator cannot well turn the wheel himself. A servant is therefore necessary, who must sit to his work. The machine only admits of one globe, or cylinder, which must have two necks; though it admits of a considerable variety of such, and it is by no means portable.

THE Abbé Nollet's machine [Plate IV. fig. 2.] resembles the greatest number of the electrical machines that were used about the time that the Leyden phial was discovered. These were the machines, heavy and unweildy as they seem, that were generally carried from place to place, when electrical experiments made a gainful business, and would bear the expence of the conveyance.

IN those early times, electricians had no idea, that it was possible to make the globe revolve too swiftly. They, therefore, made their wheels exceedingly large, and the frame of the machine proportionably strong. The globe was generally rubbed by the hand, the conductor was a bar of iron, or generally a gun barrel, suspended in silken lines from the top of the room, and the apparatus was disposed on an adjoining table.

THESE machines are now universally laid aside, being more fit for a large laboratory

than a private study. Besides, they necessarily require an assistant, and do not admit of half the variety in the disposition of the principal parts of the construction, which the variety of experiments now demands.

ABOUT the time that Mr. Boze's beatification was talked of, electricians were very desirous of exciting a very great power of electricity; though, having no method of accumulating, or preserving it, it was dispersed as soon as raised. The machine represented in Plate V. fig. 1. was a contrivance of Dr. Watson's, to whirl four large globes at a time, and unite the power of them all.

I CANNOT help regretting that no such machines as these are constructed at this day, when, by means of electrical batteries, so great a power might be preserved, and employed to the greatest purposes. I wish the Doctor would refit the machine here described, if it be yet in being, and construct a battery proportioned to it. But I should rejoice more to see a machine moving by wind or water, turning twenty or thirty globes, and charging electrical batteries adequate to them. I make no doubt but that a full charge of two or three thousand square feet of coated glass would give a shock as great as a single common flash of lightning. They are not philosophers who will say, that nothing could be gained, and no new discoveries made by such a power.

PLATE V. fig. 2. exhibits a machine which Mr. Wilson constructed, about the time above mentioned. It is much more commodious than any that had been contrived before, as all the parts are brought within a moderate compass; so that the same person may turn the wheel, and conduct the experiments.

ITS inconveniences are, that it admits but little variety of globes or cylinders, and both these and the rubber are not sufficiently distant from other bodies. The rubber is not insulated, and the conductor is unsteady. This machine has a frame standing upon the ground, but the general construction may be preserved, and the machine be made to screw to a table. Some I have seen which, by this means, were made very portable; and a box was contrived in the inside, to contain the apparatus.

OF the more modern constructions (of which there is an endless variety) the more elegant are those in which the globe is turned by tooth and pinion. This reduces the wheel work, contained in the box [*a* Pl. VI. fig. 1.] to an exceedingly small compass, and gives the workmen an opportunity of making the machine all in brass, very elegant, and portable. But I object to them, as liable to accidents, which electricians in general cannot easily repair; and I would wish philosophers to be as independent as possible of all workmen. The conductor belonging to machines of this construction is generally hung in silk,

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supported

supported either by wooden pillars in a frame, as in the figure annexed, or by two brass arms extending from the machine.

THESE machines are certainly very commodious for screwing to a table. They require no assistant, and they admit of the experiments being made in a sitting posture; which is a great recommendation of a machine, to those persons who chuse to do things with little trouble, and who are fond of a studious sedentary life. This construction admits of very little variety in the size or number of globes, and hardly of a glass vessel of any other form. But the greatest inconvenience attending it, is the upright position of the globe and rubber, whereby every thing put upon it is apt to slip down; and the rubber is not insulated.

IN the machine represented in Plate VI. fig. 2. and which was invented by Mr. Read, mathematical instrument maker at the quadrant in Knightbridge, Hyde-park, a cylinder stands perpendicular to the horizon, supported by a brass bow, which receives the upper end of the axis; and motion is given to it by means of a pulley at the lower end of the axis, and a wheel which lies parallel to the table. The conductor [*a*] is furnished with points to collect the fire, and it is screwed to the wire of a coated jar [*b*], standing in a socket, between the cylinder and the wheel. One machine of this kind I have seen, in which the cylinder and the wheel were not separated by the conductor.

THIS



THIS construction is peculiarly useful to physicians and apothecaries; and, with Mr. Lane's electrometer [c] annexed to it (the figure of which he has given me leave to insert in the drawing annexed, taken from his own machine) as many shocks as are requisite may be given, of precisely the same, and any degree of force, without any change of posture, either in the patient or the operator, who has nothing to do but turn the wheel, without so much as touching any other part of the apparatus.

WHEN this machine is used for simple electrification, and other purposes where the shock is not required; the coated jar must be taken away, and another jar, without any coating, put in its place. By this means the conductor is fixed, which is a very great advantage, and which few machines are possessed of. But these machines, besides that they admit no variety of globes or cylinders, and no insulation of the rubber, require a motion of the arm, which I should think not quite easy.

THE ingenious Dr. Ingenhoufz of Vienna, and also Mr. Ramsden, mathematical instrument maker in the Haymarket, each independent of the other, constructed a machine in which friction is not given to any kind of hollow glass vessel whatever; but to a *circular plate of glass*, generally about nine inches in diameter. This plate turns vertically, and rubs against four cushions, each an inch and a half long, placed at the opposite ends of the

the

the vertical diameter. The conductor is a brass tube, has two horizontal branches coming from it, reaching within about half an inch of the extremity of the glass, so that each branch takes off the electricity excited by two of the cushions.

THIS construction is original and ingenious, but the cushions cannot easily be insulated, and a plate of glass is much more liable to injuries than a globe, or even a cylinder.

MR. WESLEY's people, I believe, generally use a machine in which two cylinders are turned by the same wheel: but one that I saw, in the possession of a very intelligent person of that persuasion, had the cylinders and rubbers so confined in a chest, that, though it might do very well for medical uses, it was very ill adapted to the purposes of philosophy.

BUT the machine which I would advise a philosopher to construct for his own use, is that of which a drawing is given, Plate VII. This construction is the result of my best attention to this subject. I have used it above six months (how much I leave the reader to imagine) without seeing the least reason to make any alteration of consequence in it; and believe it to have almost all the advantages, which an electrical machine designed for the closet can have. The reader will, therefore, allow me to be a little longer in the description of it than I have been of the rest.

THE

THE FRAME consists of two strong boards of mahogany [*a a*] of the same length, parallel to one another, about four inches asunder; and the lower an inch on each side broader than the upper. In the upper board is a *groove*, reaching almost its whole length. One of the *pillars* [*b*], which are of baked wood, is immoveable, being let through the upper board, and firmly fixed in the lower, while the other pillar slides in the groove above mentioned, in order to receive globes or cylinders of different sizes; but it is only wanted when an axis is used. Both the pillars are perforated with *holes* at equal distances, from the top to the bottom; by means of which globes may be mounted higher or lower according to their size; and they are tall, to admit the use of two or more globes at a time, one above the other. Four of a moderate size may be used, if two be fixed on one axis: and the wheel has several grooves for that purpose.

IF a globe with only one neck be used, as in the plate, a BRASS ARM with an open socket [*c*] is necessary to support the axis beyond the pulley; and this part is also contrived to be put higher or lower, together with the brass socket in which the axis turns. The axis [*d*] is made to come quite through the pillar, that it may be turned by another handle, without the wheel, if the operator chuses. The frame, being screwed to the table, may be placed nearer to, or farther

from the wheel, as the length of the string requires, in different states of the weather. The WHEEL is fixed in a frame by itself, [e] by which it may have any situation with respect to the pulley, and be turned to one side, so as to prevent to the string from cutting itself. The hinder part of this frame is supported by a foot of its own.

THE RUBBER [f] consists of a hollow piece of copper, filled with horse hair, and covered with a basil skin. It is supported by a socket, which receives the cylindrical axis of a round plate of glass [g], the opposite part of which is inserted into the socket of a bent steel spring [h]. These parts are easily separated, so that the rubber, or the plate of glass that serves to insulate it, may be changed at pleasure. The SPRING admits of a twofold alteration of position. It may be either slipped along the groove, or moved in the contrary direction, so as to give it every desirable position with respect to the globe or cylinder; and it is, besides, furnished with a screw [i], which makes it press harder or lighter, as the operator chuses.

THE PRIME CONDUCTOR [k] is a hollow vessel of polished copper, in the form of a pear, supported by a pillar, and a firm basis of baked wood, and it receives its fire by means of a long arched wire, or rod of very soft brass [l], easily bent into any shape, and raised higher or lower, as the globe requires; and it is terminated by an open ring, in which

which are hung *some sharp pointed wires* [*m*] playing lightly on the globe when it is in motion. The body of the conductor is furnished with *holes*, for the insertion of metallic rods, to convey the fire wherever it is wanted, and for many other purposes convenient in a course of electrical experiments. The conductor is, by this means, steady, and yet may be easily put into any situation. It collects the fire perfectly well, and (what is of the greatest consequence, though but little attended to) retains it equally every where.

WHEN positive electricity is wanted, a wire, or chain, as is represented in the plate [*n*] connects the rubber with the table or the floor. When negative electricity is wanted, that wire is connected with another conductor such as that represented [*t*, Pl. II.] while the conductor in Plate VII. is connected by another wire or chain, with the table. If the rubber be made tolerably free from points, the negative power will be as strong as the positive. In this machine I do not know which is the stronger of the two.

IN short, the capital advantages of this machine are, that glass vessels, or any other electric body, of any size or form, may be used, with one neck, or two necks at pleasure; and even several of them at the same time, if required. All the essential parts of the machine, the *globe*, the *frame*, the *wheel*, the *rubber*, and *conductor*, are quite separate; and the position of them to one another may

be varied in every manner possible. The rubber has a complete insulation, by which means the operator may command either the negative or the positive power, and may change them in an instant. The conductor is steady, and easily enlarged, by rods inserted into the holes, with which it is furnished, or by the conjunction of other conductors, in order to give larger sparks, &c. The wheel may be used or not at pleasure; so that the operator may either sit, or stand to his work, as he pleases; and he may, with the utmost ease, both manage the wheel and his apparatus.

THE machine represented in [Pl. VIII.] is constructed on the same general principles with the last. It is inferior to it in one respect, that it admits only of globes or cylinders with one neck, but these are far preferable to any other; and it is much more commodious for use, as it doth not require any strong table like the other. It consists of a pillar of mahogany [*a*] standing upright on three feet. This pillar divides in two places, to receive a wheel [*b*] in the lower part of it, and in the upper part a pulley [*c*] which is turned by a leathern strap [*d*] tightened by means of a small buckle. In the center of the pulley is a strong iron spindle, turning in two firm brass sockets, fastened to each side of the pillar. In one of these sockets the extremity of the spindle turns upon a center, by means of a piece of iron [*e*] screwed into it, while the other is held tight by a brass clasp, which  
may

may be made to hold it closer, or more loosely, at pleasure, by means of a screw [*f*]. The iron spindle terminates in a male screw, answering to a female screw in the brass cap of the globe [*g*]; and by this means any globe may be taken out, and another put into the machine with very little trouble, if these parts be always made to the same pattern.

THE RUBBER [*h*] is separated from the spring [*i*] by a plate of glass [*j*], which effectually insulates it; but the chain [*k*] connects them together when positive electricity is wanted, as in the usual method of electrifying. The spring may be made to press more or less, by means of a screw [*l*]; and it may be raised higher or lower, to suit globes of different sizes, by means of a contrivance which is not represented in the plate.

THE PRIME CONDUCTOR *m*, *n*, *o*, is the same as in Pl. VII. From the same board which supports it, arises another pillar, at the top of which is Mr. Lane's electrometer; the knob of which [*p*] may be placed as near to the knob opposite to it on the prime conductor [*q*] as is desired, by means of the graduated part [*r*]. But the whole of this may be taken away when it is not wanted.

WHEN negative electricity is desired, the chain [*k*] must be removed from the rubber, and hung upon the prime conductor, so as to connect it with the table; and a short brass rod, with a knob at the end of it, must be screwed into a small socket, which will be

found in the rubber above the plate of glass. This brass rod will then serve for a negative prime conductor ; for, in this situation, when the wheel of the machine is turned, this rod, being insulated (together with the rubber, through which all the electric fire passes to the globe) will receive sparks from whatever is presented to it, and therefore electrify negatively.

As it requires some dexterity and experience to turn the machine, standing on three feet only, without shaking it ; small plates of brass, upon which the edges of heavy weights, made of lead or iron, may be placed, are fastened to two of them ; but a large board may be firmly screwed under all the feet, or various other methods may be used, whereby the pillar, which supports the machine, may stand as firm as a person chuses.



## P A R T VI.

PRACTICAL MAXIMS FOR THE USE OF  
YOUNG ELECTRICIANS.

AS the chapter I am now entering upon is professedly designed for the use of young electricians, it is hoped that the proficient will excuse my inserting a few plain and trite maxims; which, though they be superfluous with respect to him, may not be so to all my readers. The greatest electricians (who are generally those who have had the fewest instructions) may remember the time when the knowledge of a rule or maxim, which they would perhaps smile to see in a book, would have saved them a great deal of trouble and expence; and it is hoped they will not envy others acquiring wisdom cheaper than they did. In a general treatise, every man has an equal right to expect to find what he wants; and it is for the interest of the science in general, that every thing be made as easy and inviting as possible to beginners. It is this circumstance only that can increase the number of electricians, and it is from the increase of this number that we may most reasonably expect improvements in the science.

WHEN the air is dry, particularly when the weather is frosty, and when the wind is North, or East, there is hardly any electrical machine but will work very well. If the air be damp, let the room in which the machine is used be well aired with a fire, and let the globe and every thing about it be made very dry, and it may be made to work almost as well as in the best state of the air.

WHEN a tube is used, the hand should be kept two or three inches below the upper part of the rubber; otherwise the electricity will discharge itself upon the hand, and nothing will remain upon the tube for electrical purposes.

A LITTLE bees wax drawn over the surface of a tube will greatly increase its power. When the tube is in very good order, and highly excited, it will, at every stroke, throw off many pencils of rays from its surface, without the approach of any conductor, except what may float in the common atmosphere.

It has been the custom of many electricians to line their globes with sealing wax, or some other electric substance, in order to make them act with more ease and vigour. Mr. C. L. Epinasse gives the following receipt for an electric composition for this purpose. Take four pounds of Venice turpentine, one pound of rosin, and one pound of bees wax. Boil these over a gentle fire, stirring them now and then, for four hours; at the end of which stir in one quarter of a pound of vermilion. Then  
a little

a little of the mixture being taken out and left to cool, will be hard and brittle, a token that it is fit for use. Having well heated your globe or cylinder, pour the melted mixture into it, and turn it about, so as to spread it evenly over the inside surface, to the thickness of a sixpence, and let it cool very gradually\*.

I MAKE no doubt but that this electric lining is useful in some cases, especially in keeping the inside of a globe free from moisture, which is more apt to adhere to glass than other electric substances. It will be seen that a lining of sulphur was remarkably useful in the case of two large globes of my own, the history of which will be given in the last part of this work.

To increase the quantity of electric fire from a globe, moisten the rubber a little from time to time; or rather moisten the under side of a loose piece of leather, which may occasionally be put upon the rubber. But the most powerful exciter of electricity is a little amalgam, which may be made by rubbing together mercury and thin pieces of lead or tinfoil in the palm of the hand. If the rubber should be placed perpendicular to the horizon, it will be necessary to use a little tallow to make it stick. With this excellent resource, almost all states of the weather are equal to an electrician.

\* Phil. Trans. Vol. lvii. p. 186.

A LITTLE time after fresh amalgam has been put upon the globe, and often at other times, if there be any foulness upon the cushion; and sometimes when there is none, there will be formed upon the globe small black spots, of a hard rough substance, which grow continually larger, till a considerable quantity of that matter be accumulated upon the surface. This must be carefully picked off, or it will obstruct the excitation, and in a great measure defeat the electrical operations.

WHEN the amalgam has been used for some time, there will be formed upon the rubber a thick incrustation of the same kind of black substance which is apt to adhere to the globe. This incrustation is a very great improvement of the rubber. For when once a considerable body of this matter is formed, and it is a little moistened, or scraped, as much fire will be produced, as if fresh amalgam were used: so that it seems almost to supersede the farther use of the amalgam.

As the electric matter is only collected at the rubber, it is necessary that it have a communication with the common mass of the earth, by means of good conductors. If, therefore, the table on which the machine stands, or the floor of the room in which it is used, be very dry, little or no fire will be got, be the machine ever so good. In this case it will be necessary to connect the rubber, by means of chains or wires, with the floor, or even the next water, if the neighbouring ground be

be dry. This Dr. Franklin informs me he was frequently obliged to do in Philadelphia.

WHEN the electricity of a globe is very vigorous, the electric fire will seem to dart from the cushion towards the wire of the conductor. I have seen those lucid rays (which are visible in day-light) make the circuit of half the globe, and reach the wires: and they will frequently come in a considerable number, at the same time, from different parts of the cushion. The noise attending this beautiful phenomenon exactly resembles the crackling of bay leaves in the fire. Frequently these lucid arches have *radiant points*, often four or five in different parts of the same arch. These radiant points are intensely bright, and appear very beautiful. It is peculiarly pleasing to observe these circles of fire rise from those parts of the cushion where the amalgam or moisture has been put, or which have been lately scraped. Single points on the rubber will then seem intensely bright, and for a long time together will seem to pour out continual torrents of flame. If one part of the rubber be pressed closer than another, the circles will issue in that place more frequently than in any other.

WHEN the conductor is taken quite away, circles of fire will appear on both sides the rubber, which will sometimes meet, and completely encircle the globe. If a finger be brought within half an inch of the globe, in that state, it is sure to be struck very smartly, and

and there will often be a complete arch of fire from it to the rubber, though it be almost quite round the globe.

THE smaller the conductor is made, the more fire may be collected from it: for there is less surface from which the fire may escape. But in charging a phial, if the wire be placed close to the conductor, the difference will be inconsiderable, whether a smaller or larger conductor be used, till it begin to be charged pretty high; for, till that time, the conductor will not have acquired any considerable atmosphere.

IF the conductor be made perfectly well, and the air be dry, there will never be any loss of fire from any part of it. For when the whole surface has received as high a charge as the machine can give to it, it will, in all places alike, perfectly resist all farther efforts to throw more upon it, and the circulation of the fluid by the rubber will be stopped, being balanced, as it were, by equal forces. Or if it lose in all places alike, the dissipation must be invisible. This maxim almost admits of ocular demonstration. For when the rubber is perfectly insulated, and the conductor has an opportunity of discharging itself, the rubber will take sparks from a wire placed near it very fast; but when the conductor has little opportunity of emptying itself, it will take fewer of those sparks.

To form a just estimate of the electrical power of any machine, and to compare different machines in this respect, take two wires with

with knobs of any size, and fix one of them at the conductor, and the other at some certain distance from it, about an inch, or an inch and a half; and when the wheel is turned, count the number of sparks that pass between them during any given time. Fix the same wires to any other conductor, belonging to any other machine (but the same conductor would be more exact) and the difference between the number of strokes in any given time will ascertain the difference between the strength of the two machines.

THE larger the conductor is made, the stronger spark it will give: for the more extended the electrified surface is, the greater quantity of the electric atmosphere it contains, and the more sensible will be its effects when it is all discharged at once. The conductor, however, may be made so large, that the necessary dissipation of the electric matter from its surface into the air will be equal to the supply from the machine, which will constitute the MAXIMUM of the power of that machine, and will be different in different states of the air.

A CERTAIN degree of friction is necessary to give a globe its greatest power. A number of globes increases the power, but the increase of friction will make it more difficult for a man to excite their power. A few trials with a number of globes would enable any man to judge of the *maximum* of his strength in exciting electricity. I should imagine, from my own experience, that no person could excite much

much more electricity from any number of globes, than he could from one; supposing him to continue the operation an hour, or even only half an hour together.

WHEN a long conductor is used, the longest and the strongest spark may be drawn from the extremity of it, or from that part which is the most remote from the globe.

VERY large and pungent sparks are often drawn from any conductor along an electric substance. Thus if the conductor be supported by pillars of glass or of baked wood, these sparks will be taken close to the pillar.

IF the conductor bend inwards in any place, so as to make the surface concave, a peculiarly large, strong, and undivided spark may be drawn from that place. Where the surface is convex, the spark is more apt to be divided and weakened.

IF a smooth cork ball be hung in a long silken string, and electrified positively, it will always be repelled by positive, and attracted by negative electricity. But the strongest repulsion will be changed into attraction at a certain distance.

IF two pith balls hung by linen threads, and diverging with positive electricity, be insulated, though in connection with conductors of considerable length, the approach of a body electrified positively will first make them separate, and then (if the electricity of the balls be small, and that of the approaching body great) it will, at a certain distance, make them approach, and at length come in-  
to



to contact with it. Sometimes the divergence previous to the convergence is very slight, and, without great attention, is apt to be overlooked.

If the balls have a free communication with the earth, for instance, if they be held in the hand of a person standing on the ground, and (as in the former case) they be made to diverge with positive electricity, in consequence of being held within the influence of a body electrified negatively, the approach of positive electricity will make them converge, and negative electricity will make them diverge: the electric matter of the approaching body, in the former case, repelling that of the balls, and thereby, as it were, unelectrifying them; whereas, in the latter case, the negative electricity of an approaching body draws it more powerfully into the threads, and makes them diverge more. This method of judging is, therefore, excellently adapted to ascertain the kind of electricity in the atmosphere, or of a charged jar or battery, the balls being held in the hand of a person standing on the earth or the floor.

To discover smaller degrees of electricity than the balls can shew to advantage, use a very fine thread, or two of them. If insulation be necessary, fasten it to a stick of baked wood. But the most accurate measure of electricity I have yet hit upon, is a single thread of silk as it comes from the worm. When the end of this has received a small degree of electricity, it will retain it a considerable

able time, and the slightest electric force will give it motion. Before any experiments be made, let it be carefully observed how long, in any particular situation, it will retain the degree of electricity that is intended to be given to it; and let allowance be made for that in the course of the experiments. It will retain electricity much longer, if a small piece of down from a feather be fastened to it, but it will not acquire the virtue so soon. And it will be most easy to manage, if two or three threads of silk be used, and the piece of down be so adjusted to them, that it shall but just prefer a perpendicular situation, and not absolutely float in the air at random. This electrometer is not liable to the inaccuracies of those that have a sensible weight: for as there is always a sphere of attraction within a sphere of repulsion, the weight of the electrified body will allow another to pass the boundary of those two spheres, without a sensible obstruction; but the body I am describing immediately retires, with all its spheres of attraction and repulsion about it.

THE force of the ELECTRIC SHOCK is in proportion to the quantity of surface coated, the thinness of the glass, and the power of the machine. That this last circumstance ought to be taken into consideration is evident: for different machines will charge the same jar very differently. With one machine, for instance, it may be made to discharge itself, when it cannot with another.

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THE most effectual method of charging a jar is to connect the outside, by means of wires, with the rubber, while the wire proceeding from the inside is in contact with the conductor. In this manner the inside of the jar will be supplied with the very same fire that left the outside. In this case also the jar will receive as high a charge as it is capable of receiving, though the rubber be insulated, and have no communication but with the outside coating; so that, in the case of charging, there can be no occasion for the directions given above, when the table, the floor of the room, or the ground are very dry.

THE greatest quantity of fire that a jar will hold is not always the quantity it will contain when it is coated just so low as not to discharge itself. In this case, indeed, the part that is coated is charged as high as it can be, but then a considerable part of the surface is not charged at all, or very imperfectly. On the other hand, if the jar be coated very high, it may be made to discharge itself with as small an explosion as one chuses. The exact *maximum* of the charge of any jar is not easy to ascertain.

THE greatest effort in a jar to make a discharge seems to be about half a minute, or a minute, after it is removed from the conductor, owing, perhaps, to non-electric dust or moisture attracted by and adhering to the glass, between the outside and inside coating; so that if there be any apprehension of its dis-

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charging

charging itself, it is adviseable to discharge it before it has stood charged at all.

WHEN a thin jar is discharged, it is advisable not to do it by placing the discharging rod opposite to the thinnest part. It will endanger the bursting of the jar in that place.

THE more persons join hands to take a shock the weaker it is.

IF two jars, of the same thickness, be used together, the stronger of them will receive no higher a charge than the weaker. If one of them, for instance, be coated so high as that it will discharge itself, either with or without bursting, after a few turns of the wheel; the other will always be discharged along with it, though it was capable of being charged ever so high by itself. The method, therefore, of estimating the force of a number of jars, is to consider each of them as capable of containing no more fire than the weakest in the company. It follows from hence, that if a single jar in a large battery have the smallest crack in the coated part of it, not one of them is capable of being charged in conjunction with it.

IN large batteries, it is advisable to coat the jars pretty high, the dissipation of the electric matter from so great a surface when the charge is high being very considerable. The battery might be made so large, as that after a very moderate charge, the machine would be able to throw no more fire in than  
was

was exhaled, as we may say, from the surface. This would be the MAXIMUM OF THE POWER OF THAT MACHINE IN CHARGING.

IN order to judge of the strength of a charge (which, in large batteries, is a thing of considerable consequence) present Mr. Canton's balls to the wires, from time to time. A comparison of the degree of their divergence, compared with the actual explosion, will soon enable the operator to tell how high his battery is charged, and what will be the force of the explosion.

IN comparing different explosions by their power to melt wires, let it be observed, that, in wires of the same thickness, the forces that melt them will be in the proportion of the lengths; and in wires of the same length, in the proportion of the squares of their diameters.

Do not expect that the explosion of a battery will pierce a number of leaves of paper in proportion to its force in other respects. That depends upon the height of the charge much more than the quantity of coated surface. I have known an explosion which would have melted a pretty thick wire not able to pierce the cover of a book, which a small common jar would have done with ease. If it had been pierced with the explosion of the battery, the hole would have been larger in proportion.

LET no person imagine that, because he can handle the wires of a large battery without feeling any thing, that therefore he may safely touch the outside coating with one hand, while the other is upon them. I have more than once received shocks that I should not like to receive again, when the wires showed no sign of a charge; even two days after the discharge, and when papers, books, my hat, and many other things had lain upon them the greatest part of the time. If the box be tolerably dry, the residuum of the charge will not disperse very soon. I have known even the *residuum of a residuum* in my batteries to remain in them several days. For presently after an explosion, I seldom fail to discharge the residuum, which, in some cases, is very considerable, for fear of a disagreeable accident.

A SMALL shock passing through the body gives a sensation much more acute and pungent than a large one. I cannot boast, like Dr. Franklin, of being twice struck senseless by the electric shock; but I once, inadvertently, received the full charge of two jars, each containing three square feet of coated glass. The stroke could not be called painful, but, though it passed through my arms and breast only, it seemed to affect every part of my body alike. The only inconvenience I felt from it was a lassitude, which went off in about two hours.

MR.

MR. WILCKE was struck down senseless, by accidentally receiving, from his head to his feet, the charge of a large chymical receiver. He thought, that if he had received a shock five times as large, he should never have written the account of it\*.

• Remarks on Franklin's Letters, p. 362.

## P A R T VII.

## A DESCRIPTION OF THE MOST ENTERTAINING EXPERIMENTS PERFORMED BY ELECTRICITY.

**E**LECTRICITY has one considerable advantage over most other branches of science, as it both furnishes matter of speculation for philosophers, and of entertainment for all persons promiscuously. Neither the air pump, nor the orrery; neither experiments in hydrostatics, optics, or magnetism; nor those in all other branches of Natural Philosophy ever brought together so many, or so great concourses of people, as those of electricity have done singly. Electrical experiments have, in almost every country in Europe, occasionally furnished the means of subsistence to numbers of ingenious and industrious persons, whose circumstances have not been affluent, and who have had the address to turn to their own advantage that passion for the marvellous, which they saw to be so strong in all their fellow-creatures. A man need not desire a greater income than the sums which have been received in shillings, six-pences, three-pences, and two-pences, for exhibiting the Leyden experiment.

If we only consider what it is in objects that makes them capable of exciting that pleasing



ing astonishment, which has such charms for all mankind, we shall not wonder at the eagerness with which persons of both sexes, and of every age and condition, run to see electrical experiments. Here we see the course of nature, to all appearance, intirely reversed, in its most fundamental laws, and by causes seemingly the slightest imaginable. And not only are the greatest effects produced by causes which seem to be inconsiderable, but by those with which they seem to have no connection. Here, contrary to the principles of gravitation, we see bodies attracted, repelled, and held suspended by others, which are seen to have acquired that power by nothing but a very slight friction; while another body, with the very same friction, reverses all its effects. Here, we see a piece of cold metal, or even water, or ice, emitting strong sparks of fire, so as to kindle many inflammable substances; and *in vacuo* its light is prodigiously diffused and copious, so as exactly to resemble, what it really is, the lightning of heaven. Again, what can seem more miraculous than to find, that a common glass phial or jar, should, after a little preparation (which, however, leaves no visible effect, whereby it could be distinguished from other phials or jars) be capable of giving a person such a violent sensation, as nothing else in nature can give, and even of destroying animal life; and this shock attended with an explosion like thunder, and a flash like that of lightning? Lastly, what would the ancient

philosophers, what would Newton himself have said, to see the present race of electricians imitating in miniature all the known effects of that tremendous power, nay disarming the thunder of its power of doing mischief, and, without any apprehension of danger to themselves, drawing lightning from the clouds into a private room, and amusing themselves at their leisure, by performing with it all the experiments that are exhibited by electrical machines.

So far are philosophers from laughing to see the astonishment of the vulgar at these experiments, that they cannot help viewing them with equal, if not greater astonishment themselves. Indeed, all the electricians of the present age can well remember the time, when, with respect to these things, they themselves would have ranked among the same ignorant staring vulgar.

BESIDES, so imperfectly are these strange appearances understood, that philosophers themselves cannot be too well acquainted with them; and therefore should not avoid frequent opportunities of seeing the same things, and viewing them in every light. It is possible that, in the most common appearances, some circumstance or other, which had not been attended to, may strike them; and that from thence light may be reflected upon many other electrical appearances.

WHETHER philosophers may think this consideration worth attending to or not, I shall, for the sake of those electricians who  
are

are young enough, and, as it may be thought, childish enough, to divert themselves and their friends with electrical experiments, describe a number of the most beautiful and surprising appearances in electricity; that the young operator may not be at a loss what to exhibit when a company of gentlemen or ladies wait upon him, and that he may be able to perform the experiments to the most advantage, without disappointing his friends, or fretting himself.

To make this business the easier to the young operator, I shall consult his convenience in the order in which I shall relate the experiments, beginning with those which only require simple electrification, then proceeding to those in which the Leyden experiment is used, and concluding with those in which recourse must be had to other philosophical instruments in conjunction with the electrical machine.

## SECTION

## S E C T I O N I.

ENTERTAINING EXPERIMENTS IN WHICH  
THE LEYDEN PHIAL IS NOT USED.

THE phenomena of electrical attraction are shown in as pleasing a manner by the tube, as they can be by any methods that have been found out since the later improvements in electricity. It is really surprising to see a feather, or a piece of leaf gold first attracted by a glass tube excited by a slight friction, then repelled by it, and held suspended in the air, or driven about the room wherever the operator pleases; and the surprise is increased by seeing the feather, which was repelled by the smooth glass tube, attracted by an excited rough tube, or a stick of sealing wax, &c. and jumping from the one to the other, till the electricity of both be discharged. Nor is the observation of Otto Guericke the least pleasing circumstance, viz. that in turning the tube round the feather, the same side of the feather is always presented towards it.

BUT since electrical substances part with their electricity but slowly, the more rapid alternate attractions and repulsions are shown to the best advantage at the prime conductor. Thus present a number of seeds of any kind,  
grains

grains of sand, a quantity of brass dust, or other light substances in a metal dish (or rather in a glass cylindrical vessel standing on a metal plate) to another plate hanging from the conductor [as at *n* and *o*, Pl. II.] and the light substances will be attracted and repelled with inconceivable rapidity, so as to exhibit a perfect shower, which, in the dark will be all luminous.

SUSPEND one plate of metal to the conductor, and place a metal stand, of the same size, at the distance of a few inches exactly under it, and upon the stand put the figures of men, animals, or whatever else shall be imagined, cut in paper or leaf gold, and pretty sharply pointed at both extremities; and then, upon electrifying the upper plate, they will perform a dance, with amazing rapidity of motion, and to the great diversion of the spectators.

IF a downy feather, or a piece of thistle down be used in this manner, it will be attracted and repelled with such astonishing celerity, that both its form and motion will disappear; all that is to be discerned being its colour only, which will uniformly fill the whole space in which it vibrates\*.

IF a piece of leaf gold be cut with a pretty large angle at one extremity, and a very acute one at the other, it will need no lower plate, but will hang by its larger angle at a small distance from the conductor, and by the con-

\* Lovet, p. 28.

tinual waving motion of its lower extremities, will have the appearance of something animated, biting or nibbling at the conductor. It is therefore called by Dr. Franklin the *golden fish*.

To the dancing figures above mentioned, it is very amusing to add a set of ELECTRICAL BELLS. These consist of three small bells, the two outermost of which are suspended from the conductor by chains, and that in the middle by a silken string, while a chain connects it with the floor; and two small knobs of brass, to serve instead of clappers, hang by silken strings, one between each two bells. In consequence of this disposition, when the two outermost bells, communicating with the conductor, are electrified, they will attract the clappers, and be struck by them. The clappers, being thus loaded with electricity, will be repelled, and fly to discharge themselves upon the middle bell. After this, they will be again attracted by the outermost bells; and thus by striking the bells alternately, a continual ringing may be kept up as long as the operator pleases. In the dark, a continual flashing of light will be seen between the clappers and the bells; and when the electrification is very strong, these flashes of light will be so large, that they will be transmitted by the clapper from one bell to the other, without its ever coming into actual contact with either of them, and the ringing will, consequently, cease. When these two experiments of the bells and the figures are exhibited

ed at the same time, they have the appearance of men or animals dancing to the music of the bells ; which, if well conducted, may be very diverting.

IF a piece of burnt cork, about the bigness of a pea, cut into the form of a spider, with legs of linen thread, and a grain or two of lead put in it, to give it more weight, be suspended by a fine silken thread, it will, like a clapper between the two bells, jump from an electrified to an unelectrified body and back again, or between two bodies possessed of different electricities, moving its legs as if it were alive, to the great surprize of persons unacquainted with the construction of it. This is an American invention, and is described by Dr. Franklin \*.

SEVERAL very beautiful experiments, which depend on electrical repulsion, may be shown to great advantage by bundles of thread, or of hair, suspended from the conductor, or presented to it. They will suddenly start up, and separate upon being electrified, and instantly collapse when the electricity is taken off. If the operator can manage this experiment with any degree of dexterity, the hair will seem to the company to rise and fall at the word of command.

IF a large plummy feather be fixed upright on an electrified stand, or held in the hand of a person electrified, it is very pleasing to observe how it becomes turgid, its fibres extend-

\* Letters, p. 17.

ing themselves in all directions from the rib ; and how it shrinks, like the sensitive plant, when any unelectrified body touches it, when the point of a pin or needle is presented to it, or when the prime conductor with which it is connected.

BUT the effects of electrical repulsion are shown in a more surprising manner by means of water issuing out of a capillary tube. If a vessel of water be suspended from the conductor, and a capillary syphon be put into it, the water will issue slowly, and in the form of large drops from the lower leg of the syphon ; but, upon electrifying this little apparatus, instead of drops, there will be one continued stream of water ; and if the electrification be strong, a number of streams, in the form of a cone, the apex of which will be at the extremity of the tube ; and this beautiful shower will be luminous in the dark.

LASTLY, Mr. Rackstrow's experiment (as it is generally called, but which Mr. Henley informs me was really invented by John Serocold, Esq.) is a striking instance of electrical attraction and repulsion, and, at the same time, exhibits a very pleasing spectacle. Electrify a hoop of metal, suspended from the prime conductor (or supported with small pieces of sealing wax, &c.) about half an inch above a plate of metal, and parallel to it. Then place a round glass bubble, blown very light, upon the plate, near the hoop, and it will be immediately attracted to it. In consequence of this, the part of the bubble which  
touched



touched the hoop will acquire some electric virtue, and be repelled; and, the electricity not being diffused over the whole surface of the glass, another part of the surface will be attracted, while the former goes to discharge its electricity upon the place. This will produce a revolution of the bubble quite round the hoop, as long as the electrification is continued; and it will be either way, just as it happens to set out, or as it is driven by the operator. If the room be darkened the glass ball will be beautifully illuminated. Two bubbles may be made to revolve about the same hoop, one on the inside, and the other on the outside; and either in the same, or contrary directions. If more hoops be used, a greater number of bubbles may be made to revolve, and thus a kind of *planetarium* or *orrery* might be constructed, and a ball hung over the center of all the hoops would serve to represent the sun in the center of the system. Or the hoops might be made elliptical, and the sun be placed in one of the *foci*. N. B. A bell or any metallic vessel inverted would serve instead of a single hoop.

ALL the motions above mentioned are the immediate effect of electrical attraction and repulsion. The following amusing experiments are performed by giving motion to bodies through the medium of air, i. e. by first putting the air in motion. Let the electrician provide himself with a set of vanes, made of gilt paper or tinsel, each about two inches in length and one in breadth. Let these be  
stuck

stuck in a cork, which may be suspended from a magnet by means of a needle; and then, if they be held at a small distance on one side of the end of a pointed wire proceeding from the conductor, they will be turned round with great rapidity by the current of the air which flows from the point. If the vanes be removed to the other side of the point, the motion will presently stop, and begin again with the same rapidity, in a contrary direction; and thus the motion may be changed at pleasure. This experiment may be diversified by vanes cut in the form of those of a smoke-jack; when, being held over the end of a pointed wire, turned upwards, and electrified, they will be turned round very swiftly, by the current of air flowing upwards. If they be held under a point projecting downwards, they will be turned the contrary way.

ON the top of a finely pointed wire, rising perpendicularly from the conductor, let another wire, sharpened at each end, be made to move freely as on a center. If it be well balanced, and the points be bent horizontally, in opposite directions, it will, when electrified, turn very swiftly round, by the reaction of the air against the current which flows from the points. These points may be nearly concealed, and horses or other figures placed upon the wires, so as to turn round with them, and look as if the one pursued the other. This experiment Mr. Kinnerley calls the ELECTRICAL HORSE RACE. If the number of wires proceeding from the same center be increased,

creased, and different figures be put upon them, the race will be more complicated and diverting. If this wire which supports the figures have another wire finely pointed rising from its center, another set of wires, furnished with other figures, may be made to revolve above the former, and either in the same, or in a contrary direction, as the operator pleases.

If such a wire, pointed at each end, and the ends bent in opposite directions, be furnished like a dipping needle with a small axis fixed in its middle, at right angles with the bending of the points, and the same be placed between two insulated wire strings, near and parallel to each other, so that it may turn on its axis freely upon and between them; it will, when electrified, have a progressive as well as circular motion, from one end of the wires that support it to the other, and this even up a considerable ascent.

A VARIETY of beautiful appearances may be exhibited by means of electrical LIGHT, even in the open air, if the room be dark. Brushes of light from points electrified positively, and not made very sharp, or from the edges of metallic plates, diverge in a very beautiful manner, and may be excited to a great length, by presenting to them a finger, or the palm of the hand, to which they feel like soft lambent flames, which have not the least pungency, nor give a disagreeable sensation of any kind. It is also amusing to observe the difference there is between brushes

of light from pointed bodies electrified positively or negatively.

IN the electrical horse race above mentioned, a small flame will be seen in the dark at every point of the bent wires ; so that, if the operator can contrive to make the wire terminate in the horse's tail, it will seem to be all on fire. And if a circular plate of metal be cut into the shape of a star, so that every point may be at the same distance from the center, and the center be made to turn freely on a point, like the wires in the preceding experiment, a small flame will be seen at every point ; and if the star be turned round, it will exhibit the appearance of a lucid circle, without any discontinuance of the light.

IF the electric sparks be taken from a brass ball, at the extremity of a long brass rod, inserted into the prime conductor, they will often be several inches long, and issue in a great variety of crooked directions, exactly resembling the course of lightning, and exhibiting a very amusing spectacle. A friend of Dr. Franklin's supposes that the spark is thrown out of a strait course by the density of the air increased by the action and reaction of the two fluids, which are repulsive of one another\*.

As the motion of the electric matter is, to the senses, instantaneous, a variety of beautiful appearances may be exhibited by a num-

\* Franklin's Letters, new edition, p. 167.

ber of small electric sparks, disposed in various forms. This may be done by means of a board and a number of wires, in the following manner. Let two holes be made through the board, about a quarter of an inch on each side of the spot where a spark is desired. Let the extremities of the wires neatly rounded, come through these holes, and be brought near together, exactly over the place; and let the wires on the back side of the board be so disposed, as that an electric spark must take them all in the same circuit. When they are thus prepared, all the points will appear luminous at once, whenever a spark is taken by them at the prime conductor. In this manner may beautiful representations be made of any of the constellations, as of the Great Bear, Orion, &c. and in this manner, also, may the outlines of any drawing, as of figures in tapestry, be exhibited.

THE Abbé Nollet has taken a great deal of pains to make the appearance of letters, and other figures, by means of electric sparks, and as it is impossible to make the sparks follow one another in a complete circle, on the same side of any flat surface, he makes use of plates of glass, and places one half of the circle, &c. on one side of the plate, and the other half on the other side, connecting the pieces that are nearest one another, but on different sides of the glass, by wires brought round it\*. The description would be too tedious for this

\* Lettres, Vol. iii. p. 281.

place, but the execution will be very easy, to any person who has but a little knowledge of electricity.

THE force of an electric spark in setting fire to various substances was one of the first experiments that gave an *eclat* to electricity, and it is still repeated with pleasure. Spirit of wine a little warmed, is commonly made use of for this purpose. The experiment will not fail to succeed, if a pretty strong spark be drawn, in any manner, or direction whatever, through any part of it; and this may easily be done many ways, if it be contained in a metal spoon with a pretty wide mouth. A candle newly blown out may be lighted again by the electric spark passing through the gross part of the smoke, within half an inch of the snuff; though it is perhaps blown in again by the motion given to the air by the force of the explosion. Also air produced by the effervescence of steel filings with oil of vitrol diluted with water, and many other substance, which throw out an inflammable vapour, may be kindled by it.

THE strong phosphoreal or sulphureous smell, which may be perceived by presenting the nostrils within an inch or two of any electrified point, makes a curious experiment, but it does not give a pleasing sensation.

LASTLY, the most entertaining experiment that can be performed by simple electrification, is when one or more of the company stand upon an insulated stool, holding a chain from the prime conductor. In this case, the whole  
body

body is, in reality, a part of the prime conductor, and will exhibit all the same appearances, emitting sparks wherever it is touched by any person standing on the floor. If the prime conductor be very large, the sparks may be too painful to be agreeable, but if the conductor be small, the electrification moderate, and none of the company present touch the eyes, or the more tender parts of the face of the person electrified, the experiment is diverting enough to all parties.

MOST of the experiments above mentioned may also be performed to the most advantage by the person standing upon the stool, if he hold in his hand whatever was directed to be fastened to the prime conductor. Spirit of wine may be fired by a spark from a person's finger as effectually as in any other way. Care must be taken that the floor on which the stool is placed be free from dust, but it is most advisable to have a large smooth board for the purpose.

## SECTION II.

## ENTERTAINING EXPERIMENTS PERFORMED BY MEANS OF THE LEYDEN PHIAL.

NO electrical experiments answer the joint purpose of pleasure and surprize in any manner comparable to those that are made by means of the Leyden phial. All the varieties of electrical attraction and repulsion may be exhibited, either by the wire, or the coating of it; and if the knobs of two wires, one communicating with the inside, and the other with the outside of the phial, be brought within four or five inches of one another, the electrical spider above mentioned will dart from the one to the other in a very surprising manner, till the phial be discharged. But the peculiar advantage of the Leyden experiment is, that, by this means, the electrical flash, report, and sensation, with all their effects, may be increased to almost any degree that is desired.

WHEN the phial, or the jar, is charged, the shock is given through a person's arms and breast, by directing him to hold a chain communicating with the outside in one hand, and to touch the wire of the phial, or any conductor communicating with it, with the other



other hand. Or the shock may be made to pass through any particular part of the body without much affecting the rest, if that part, and no other, be brought into the circuit through which the fire must pass from one side of the phial to the other.

A GREAT deal of diversion is often occasioned by giving a person a shock when he does not expect it; which may be done by concealing the wire that comes from the outside of the phial under the carpet, and placing the wire which comes from the inside in such a manner in a person's way, that he can suspect no harm from putting his hand upon it, at the same time that his feet are upon the other wire. This, and many other methods of giving a shock by surprize, may easily be executed by a little contrivance; but great care should be taken that these shocks be not strong, and that they be not given to all persons promiscuously.

WHEN a single person receives the shock, the company is diverted at his sole expence; but all contribute their share to the entertainment, and all partake of it alike, when the whole company forms a circuit, by joining their hands; and when the operator directs the person who is at one extremity of the circuit to hold a chain which communicates with the coating, while the person who is at the other extremity of the circuit touches the wire. As all the persons who form this circuit are struck at the same time, and with the

same degree of force, it is often very pleasant to see them start at the same moment, to hear them compare their sensations, and observe the very different accounts they give of it.

THIS experiment may be agreeably varied, if the operator, instead of making the company join hands, direct them to tread upon each others toes, or lay their hands upon each others heads; and if, in the latter case, the whole company should be struck to the ground, as it happened when Dr. Franklin once gave the shock to six very stout robust men, the inconvenience arising from it will be very inconsiderable. The company which the Doctor struck in this manner neither heard nor felt the stroke, and immediately got up again, without knowing what had happened. This was done with two of his large jars (each containing about six gallons) not fully charged\*.

THE most pleasing of all the surprizes that are given by the Leyden phial is that which Mr. Kinnerley † contrived and called the MAGIC PICTURE, which he describes in the following manner. Having a large metzo-tinto, with a frame and glass (suppose of the king) take out the print, and cut a pannel out of it, near two inches distant from the frame all round. If the cut be through the

\* Franklin's Letters, new edition, p. 324.

† Ibid. p. 29.

picture, it is not the worse. With thin paste, or gum water, fix the board that is cut off on the inside of the glass, pressing it smooth and close: then fill up the vacancy, by gilding the glass well with leaf gold, or brass. Gild likewise the inner edge of the back of the frame all round, except the top part, and form a communication between that gilding and the gilding behind the glass; then put in the board, and that side is finished. Turn up the glass, and gild the foreside exactly over the back gilding; and when it is dry, cover it, by pasting on the pannel of the picture that has been cut out, observing to bring the correspondent parts of the board and picture together, by which the picture will appear of a piece as at first, only part is behind the glass, and part before. Lastly, hold the picture horizontally by the top, and place a little moveable gilt crown, on the king's head

If now the picture be moderately electrified, and another person take hold of the frame with one hand, so that his fingers touch its inside gilding, and with the other hand endeavour to take off the crown, he will receive a terrible blow, and fail in the attempt. The operator who holds the picture by the upper end, where the inside of the frame is not gilt, to prevent its falling, feels nothing of the shock, and may touch the face of the picture without danger, which he pretends to be a test of his loyalty. If a  
ring

ring of persons take a shock among them, the experiment is called the CONSPIRATORS.

As the electric fire may be made to take whatever circuit the operator shall please to direct, it may be thrown into a great variety of beautiful forms. Thus, if a charged phial be placed at one extremity of the gilding of a book, and the discharge be made by a wire which touches the other extremity, the whole gilding will be rendered luminous. But if several pretty strong shocks be sent through the same gilding, they will soon render it incapable of transmitting any more, by breaking and separating the parts too far asunder. Also the electric constellations and figures, mentioned above, may be lighted up much more strongly by a charged phial than by sparks from the conductor; only, they cannot be lighted up so often in this way.

ON the same principle that the wires of phials charged differently will attract and repel differently, is made an ELECTRICAL WHEEL, which Dr. Franklin says, turns with considerable strength, and of which he gives the following description. A small upright shaft of wood passes at right angles through a thin round board, of about twelve inches diameter, and turns on a sharp point of iron, fixed in the lower end; while a strong wire in the upper end, passing through a small hole in a thin brass plate, keeps the shaft truly vertical. About thirty *radii*, of equal length,

2                      made

made of fash glafs, cut in narrow flips, iffue horizontally from the circumference of the board; the ends moft diftant from the center being about four inches apart. On the end of every one a brafs thimble is fixed.

IF now the wire of a bottle, electrified in the common way, be brought near the circumference of this wheel, it will attract the neareft thimble, and fo put the wheel in motion. That thimble, in paffing by, receives a fpark, and thereby being electrified, is repelled, and fo driven forwards, while a fecond being attracted approaches the wire, receives a fpark, and is driven after the firft; and fo on, till the wheel has gone once round; when the thimbles before electrified approaching the wire, inftead of being attracted, as they were at firft, are repelled, and the motion prefently ceafes.

BUT if another bottle, which had been charged through the coating, be placed near the fame wheel, its wire will attract the thimble repelled by the firft, and thereby double the force that carries the wheel round; and not only take out the fire that had been communicated by the thimbles to the firft bottle, but even robbing them of their natural quantity, inftead of being repelled when they come again towards the firft bottle, they are more ftrongly attracted; fo that the wheel mends its pace, till it goes with great rapidity, twelve or fifteen rounds in a minute, and  
with

with such strength, that the weight of one hundred Spanish dollars, with which we once loaded it, did not in the least seem to retard its motion. This is called an ELECTRICAL JACK, and if a large fowl was spitted on the upper shaft, it would be carried round before a fire, with a motion fit for roasting.

BUT this wheel, continues the Doctor, like those driven by wind, moves by a foreign force, to wit that of the bottles.

THE SELF MOVING WHEEL, though constructed on the same principles, appears more surprising. It is made of a thin round plate of window glass, seventeen inches diameter, well gilt on both sides, all but two inches next the edge. Two small hemispheres of wood are then fixed with cement to the middle of the upper and under sides, centrally opposite; and in each of them a thick strong wire, eight or ten inches long, which together makes the axis of the wheel. It turns horizontally, on a point at the lower end of its axis, which rests on a bit of brass, cemented within a glass salt cellar. The upper end of its axis passes through a hole in a thin brass plate, cemented to a long and strong piece of glass; which keeps it six or eight inches distant from any non-electric, and has a small ball of wax or metal on its top, to keep in the fire.

IN a circle on the table which supports the wheel, are fixed twelve small pillars of glass,  
at

at about eleven inches distance, with a thimble on the top of each. On the edge of the wheel is a small leaden bullet, communicating by a wire with the gilding of the upper surface of the wheel; and about six inches from it, is another bullet, communicating, in like manner, with the under surface. When the wheel is to be charged by the upper surface, a communication must be made from the under surface to the table.

WHEN it is well charged, it begins to move. The bullet nearest to a pillar moves towards the thimble on that pillar, and, passing by, electrifies it, and then pushes itself from it. The succeeding bullet, which communicates with the other surface of the glass, more strongly attracts that thimble, on account of its being electrified before by the other bullet, and thus the wheel increases its motion, till the resistance of the air regulates it. It will go half an hour, and make, one minute with another, twenty turns in a minute, which is 600 turns in the whole, the bullet of the upper surface giving in each turn twelve sparks to the thimbles, which makes 7200 sparks, and the bullet of the under surface receiving as many from the thimble, those bullets moving in the time near 2500 feet. The thimbles are well fixed, and in so exact a circle, that the bullets may pass within a very small distance of each of them.

If instead of two bullets, you put eight, four communicating with the upper surface, and

and four with the under surface, placed alternately (which eight, at about six inches distance, complete the circumference) the force and swiftness will be greatly increased, the wheel making fifty turns in a minute, but but then it will not continue moving so long.

THESE wheels, the Doctor adds, may be applied perhaps to the ringing of chimes, and moving light made orreries\*.

A PHIAL makes the most beautiful appearances when it is charged without any coating on the outside, by putting the hand, or any conductor, to it: for then, at whatever part of the jar the discharge is made, the fire will be seen to branch from it in most beautiful ramifications all over the jar, and the light will be so intense, that the minutest of the branches may be seen in open day-light.

THE discharge of a large electrical battery is rather an awful than a pleasing experiment, and the effects of it, in rending various bodies, in firing gun-powder, in melting wires, and in imitating all the effects of lightning, never fail to be viewed with astonishment. In order to fire gun-powder, it must be made up into a small cartridge, with blunt wires inserted at each end, and brought within half an inch of each other, through which the shock must pass: or a very small wire may be drawn through the center of it, and the ex-

\* Franklin's Letters, p. 28, &c.

plosion



plosion will be made by its melting. A common jar will easily strike a hole through a thick cover of a book, or many leaves of paper, and it is curious to observe the bur raised on both sides, as if the fire had darted both ways from the center.

A CONSIDERABLE number of experiments with an electrical battery some of which exhibit fine appearances, will be particularly described in the last part of the work.

## SECTION

## S E C T I O N    III.

ENTERTAINING EXPERIMENTS MADE BY  
A COMBINATION OF PHILOSOPHI-  
CAL INSTRUMENTS.

**I**N order to exhibit some of the finest electrical experiments, the operator must call to his aid other philosophical instruments, particularly the condensing machine, and the air pump.

IF the fountain made by condensed air be insulated, and be made to emit one stream, that stream will be broken into a thousand, and equally dispersed over a great space of ground, when the fountain is electrified ; and by only laying a finger upon the conductor, and taking it off again, the operator may command either the single stream, or the divided stream at pleasure. In the dark, the electrified stream appears quite luminous.

THE greatest quantity of electric light is seen *in vacuo*. Take a tall receiver very dry, and in the top of it insert with cement a wire not very acutely pointed. Then exhaust the receiver, and present the knob of the wire to the conductor, and every spark will pass through the vacuum in a broad stream of light, visible through the whole length of the receiver, be it ever so tall. This stream often  
divides

divides itself into a variety of beautiful rivulets, which are continually changing their course, uniting and dividing again, in a most pleasing manner. If a jar be discharged through this vacuum, it gives the appearance of a very dense body of fire, darting directly through the center of the vacuum, without ever touching the sides; whereas, when a single spark passes through, it generally goes more or less to the side, and a finger put on the outside of the glass will draw it wherever a person pleases. If the vessel be grasped by both hands, every spark is felt like the pulsation of a great artery, and all the fire makes towards the hands. This pulsation is felt at some distance from the receiver; and in the dark, a light is seen betwixt the hands and the glass.

ALL this while the pointed wire is supposed to be electrified positively; if it be electrified negatively, the appearance is remarkably different. Instead of streams of fire, nothing is seen but one uniform luminous appearance, like a white cloud, or the milky way in a clear star-light night. It seldom reaches the whole length of the vessel, but is generally only like a lucid ball at the end of the wire.

A VERY beautiful appearance of electric light in a darkened room may also be produced by inserting a small phial into the neck of a tall receiver, so that the external surface of the glass may be exposed to the vacuum. The

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phial

phial must be coated on the inside, and while it is charging, at every spark taken from the conductor into the inside, a flash of light is seen to dart, at the same time, from every part of the external surface of the jar, so as quite to fill the receiver. Upon making the discharge, the light is seen to return in a much closer body, the whole coming at once.

BUT the most beautiful of all the experiments that can be exhibited by the electric light is Mr. Canton's *AURORA BOREALIS*, of which the following is but an imperfect description. Make a Torricellian vacuum in a glass tube, about three feet long, and seal it hermetically, whereby it will be always ready for use. Let one end of this tube be held in the hand, and the other applied to the conductor, and immediately the whole tube will be illuminated, from end to end; and when taken from the conductor, will continue luminous without interruption for a considerable time, very often above a quarter of an hour. If, after this, it be drawn through the hand either way, the light will be uncommonly intense, and without the least interruption from one hand to the other, even to its whole length. After this operation, which discharges it in a great measure, it will still flash at intervals, though it be held only at one extremity, and quite still; but if it be grasped by the other hand, at the same time, in a different place,

place, strong flashes of light will hardly ever fail to dart from one end to the other; and this will continue twenty four hours, and perhaps much longer, without fresh excitation. Small and long glass tubes exhausted of air, and bent in many irregular crooks and angles, will, when properly electrified in the dark, beautifully represent flashes of lightning.

I SHALL conclude this description of entertaining experiments with an account of the manner in which Dr. Franklin and his friends closed the year 1748. The hot weather coming on, when electrical experiments were not so agreeable, they put an end to them for that season, as the Doctor says, somewhat humorously, in a party of pleasure on the banks of the Skuylkil. First, spirits were fired by a spark sent from side to side through the river, without any other conductor than the water. A turkey was killed for their dinner by the electrical shock, and roasted by the electrical jack, before a fire kindled by the electrified bottle, when the healths of all the famous electricians in England, Holland, France, and Germany, were drunk in electrified bumpers, under a discharge of guns from the electrical battery\*.

HAPPY would the author of this treatise be to see all the great electricians of Europe,

\* Franklin's Letters, p. 35.

or even those in England, upon such an occasion, and especially after having made discoveries in electricity of equal importance with those made in Philadelphia in the year referred to. With pleasure would he obey a summons to such a rendezvous, though it were to serve the illustrious company in the capacity of operator, or even in the more humble office of waiter. Chearfulness and social intercourse do, both of them, admirably suit, and promote the true spirit of philosophy.

PART

## P A R T VIII.

NEW EXPERIMENTS IN ELECTRICITY,  
MADE CHIEFLY IN THE YEAR 1766.

**I** SHALL, in the last part of this work, present my reader with an account of such new experiments in electricity as this undertaking has led me to make. I hope the perusal of this work may suggest many more, and more considerable ones to my readers, and then I shall not think that I written in vain.

To make this account the more useful to such persons as may be willing to enter into philosophical investigations, I shall not fail to report the real views with which every experiment was made, false and imperfect as they often were. I was always greatly pleased with the extreme exactness and simplicity of Mr. Grey, and shall, therefore, imitate his artless manner. And though an account of experiments drawn up on this plan be less calculated to do an author honour as a philosopher; it will, probably, contribute more to make other persons philosophers, which is a thing of much more consequence to the public.

MANY modest and ingenious persons may be engaged to attempt philosophical investigations, when they see, that it requires no more sagacity to find new truths, than they themselves are masters of; and when they see that many discoveries have been made by mere accident, which may prove as favourable to them as to others. Whereas it is a great discouragement to young and enterprising geniuses, to see philosophers proposing that first, which they themselves attained to last; first laying down the propositions which were the result of all their experiments, and then relating the facts, as if every thing had been done to verify a true preconceived theory.

THIS *synthetic* method is, certainly, the most expeditious way of making a person understand a branch of science; but the *analytic* method, in which discoveries were actually made, is most favourable to the progress of knowledge.

I HAVE, indeed, endeavoured to make the whole preceding history of electricity useful in this view, by not contenting myself with informing the reader what discoveries have been made; but, wherever it could be done, acquainting him *how* they were made, and what the authors of them had in view when they made them. In general, this has not been difficult to do, the facts being recent, and most of the persons concerned now living. And, perhaps, in no branch of science  
has



has there been less owing to genius, and more to accident ; so that no person, who will give a little attention to the subject, need be without hopes of adding something to the common stock of electrical discoveries. Nay, it would be extraordinary, if, in a great number of experiments, in which things were put into a variety of new situations, no new fact, worth communicating to the public, should arise.

THE method I propose will, likewise, give, the most pleasure to those persons, who delight in tracing the real progress of the human mind, in the investigation of truth, and the acquisition of knowledge ; as I hope it will carry with it sufficient evidence of its own authenticity. For this progress, we may assure ourselves, has, in all cases, been by easy steps, even when it has been the most rapid. Were it possible to trace the succession of ideas in the mind of Sir Isaac Newton, during the time that he made his greatest discoveries, I make no doubt but our amazement at the extent of his genius would a little subside. But if, when a man publishes discoveries, he, either through design, or through habit, omit the intermediate steps by which he himself arrived at them ; it is no wonder that his speculations confound others, and that the generality of mankind stand amazed at his reach of thought. If a man ascend to the top of a building by the help of a common ladder, but cut away most of the steps after he has done with them, leaving only every

ninth or tenth step; the view of the ladder, in the condition in which he has been pleased to exhibit it, gives us a prodigious, but an unjust idea of the man who could have made use of it. But if he had intended that any body should follow him, he should have left the ladder as he constructed it, or perhaps as he found it, for it might have been a mere accident that threw it in his way. It is possible he had even better have destroyed it entirely; as, in some cases, a person would more easily make a new ladder of his own, than repair an old and damaged one.

THAT Sir Isaac Newton himself owed something to a casual turn of thought, the history of his astronomical discoveries informs us; and where we see him most in the character of an experimental philosopher, as in his optical inquiries (though the method of his treatise on that subject is by no means purely analytical) we may easily conceive that many persons, of equal patience and industry (which are not called qualities of the understanding) might have done what he did. And were it possible to see in what manner he was first led to those speculations, the very steps by which he pursued them, the time that he spent in making experiments, and all the unsuccessful and insignificant ones that he made in the course of them; as our pleasure of one kind would be increased, our admiration would probably decrease. Indeed he himself used candidly to acknowledge, that if he had done more than other men, it was owing rather to

to a habit of *patient thinking*, than to any thing else.

I DO not say these things to detract from the merit of the great Sir Isaac Newton; but I think that the interests of science have suffered by the excessive admiration and wonder, with which several first rate philosophers are considered; and that an opinion of the greater equality of mankind, in point of genius, and powers of understanding, would be of real service in the present age. It would bring more labourers into the common field; and something more, at least, would certainly be done in consequence of it. For though I by no means think that philosophical discoveries are at a stand, I think the progress might be quickened, if studious and modest persons, instead of confining themselves to the discoveries of others, could be brought to entertain the idea, that it was possible to make discoveries themselves. And, perhaps, nothing would tend more effectually to introduce that idea, which is at present very remote from the minds of many, in which it ought to have a place, than a faithful history of the manner in which philosophical discoveries have actually been made by others.

THAT this fidelity has been preserved in the following narrative, I make no doubt of its being its own voucher. Its imperfections will be a sufficient evidence. The same fidelity will also oblige me to relate several facts as appearing new to myself, which the course of the preceding history will show to have been

been discovered by others, though I was not then aware of it. Of such after-discoveries, however, I have mentioned only those which, it will be seen, I have pursued something farther than the original authors, having attended to circumstances overlooked by them; or, at least, having made the experiments with more exactness, so that the reader may expect something really new under every article. And the experiments which prove the same thing will be found considerably different from those of others, and to furnish additional arguments of the same general propositions. This repetition of old discoveries, and this variety in the experiments by which they were made, were both occasioned by a situation which is more or less common to every electrician in England; whereby we are ignorant of a great deal of what has been done by others.

IN the following narrative will also be found an account not only of experiments which are complete, which exhibit some new fact, and from which something relating to the general theory of electricity may be deduced; but also some that are incomplete, which produced no new appearance, and from which nothing positive could be concluded. If electricians in general had done this, they would have saved one another a great deal of useless labour, and would have had more time for making experiments really new, and which might have terminated in considerable discoveries. Besides, if things be really put into new situations, though nothing *positive* can be inferred from

from the experiment, at least something negative may; and this cannot be said to be of no importance in science; nor, strictly speaking, to be no new truth. A sufficient number of these experiments may, in many cases, lay a foundation for probable and positive conclusions.

I MAKE no apology for leaving so many of these experiments imperfect, and for publishing this account of them before they have been pursued so far as it may, perhaps, be thought they deserve. I rather think the generality of philosophers ought to make an apology to the public, for delaying the communication of their experiments and discoveries so long as they have done. It is possible I may never have any more leisure or opportunity to pursue them, and others may better command both; whereby the discoveries will be sooner brought to their maturity, and the progress of this branch of philosophy accelerated. The genuine spirit of philosophy is, surely, not that of mechanics, who make the most of every little improvement in their arts, and never divulge them, till they can make no more advantage of them themselves. If I could this day communicate to any fellow-labourer a hint, which it was more probable he could immediately pursue to advantage than myself, I would not defer it till to-morrow. Nor do I think it is any great boast of a philosophical indifference to fame to make this declaration. The great Sir Isaac Newton seems to have had no idea of the pursuit of fame.

fame. He deferred the communication of his important discoveries through real modesty, thinking it impertinent to trouble the public with any thing imperfect. I make no pretensions to that kind of modesty. Whether it be of a true or a false kind, I think it manifestly injurious to the progress of knowledge. Like those who contended in one of the games of ancient Greece, I shall immediately deliver my torch to any person who can carry it with more dexterity. If others do the same, it may come into my hands again, several times, before we reach the goal.

IT may be said, that I ought, at least, to have waited till I had seen the connection of my new experiments with those that were made before, and have shown that they were agreeable to some general theory of electricity. But when the facts are before the public, others are as capable of showing that connection, and of deducing a general theory from them as myself. If but the most inconsiderable part, of the temple of science be well laid out, or a single stone proper for, and belonging to it be collected; though at present it be ever so much detached from the rest of the building, its connection and relative importance will appear in due time, when the intermediate parts shall be completed. Every *fact* has a real, though unseen connection with every other fact: and when all the facts belonging to any branch of science are collected, the system will form itself. In the mean time, our guessing at the system may be some guide

guide to us in the discovery of the facts; but, at present, let us pay no attention to the system in any other view; and let us mutually communicate every new fact we discover, without troubling ourselves about the system to which it may be reduced.

I THINK I shall give the most distinct view of the few things that I have observed, in the short course of my electrical experiments, if I relate them pretty nearly in the order in which they occurred, only taking care not to intermix things of a very different nature. The earliest date in my experiments is the beginning of the year 1766; when, in consequence of forming an acquaintance with some gentlemen who have distinguished themselves for their discoveries in electricity, and of undertaking to write the preceding history, my attention was first turned towards making some original experiments in this part of Natural Philosophy, which had served for my occasional amusement some time before.

## SECTION

## SECTION I.

## EXPERIMENTS ON EXCITATION, PARTICULARLY OF TUBES IN WHICH AIR IS CONDENSED, AND OF LARGE GLASS GLOBES.

**F**INDING by my own experiments, and those of others, that a glass tube, out of which the air was exhausted, discovered no sign of electricity outwards, but that all its effects were observed on the inside; I imagined that, if the air was condensed in the tube, it would operate more strongly on the outside; so that an additional atmosphere would give it a double virtue. But the result was the very reverse of my expectations.

SOME time in the month of January, when the weather was dry and frosty, I took a glass tube, such as is generally used for electrifying, about two feet and a half in length, and an inch in diameter. It was closed at one end, and by means of a brass cap at the other, I fitted a condensing engine to it; and when the tube was very dry, and in excellent order for making experiments, I began to throw in more air. At every stroke of the piston I endeavoured to excite the tube, but found its virtue diminished. It was obliged to



to be brought nearer than before to attract light bodies, and gave less light when rubbed in the dark; till, as near as I could judge, I had got one additional atmosphere into the tube, when its power was scarce discernible. Letting out the air by degrees, I observed it gradually recovered its power. It attracted light bodies at a greater distance; it gave louder snappings, and more light in the dark; and when the additional air was wholly let out, its power was immediately as great as it had been before any air was thrown in. This I tried several times with the same success.

COMMUNICATING these experiments to Dr. Franklin and Dr. Watson, they suggested to me, that the non-excitation of the tube above mentioned might be owing to moisture introduced along with the air, and adhering to the inside of the tube. This conjecture was rendered more probable by another experiment I had made in the mean time.

REPEATING my attempts to excite the tube above mentioned, I found that, after very hard rubbing, it began to act a little; and that its virtue encreased with the labour. Thinking it might be the warmth which produced this effect, I held the tube to the fire, and found that when it was pretty hot, it would act almost as well as when it contained no more than its usual quantity of air. I conjectured that the warmth might expel the moisture from the sides of the glass, or make the  
enclosed

enclosed air capable of holding a greater quantity of water in a state of perfect solution.

WILLING to determine whether the additional quantity of air with the moisture, it occasioned, acted in all respects like a non-electric coating, I tried the experiment with condensed air, that Dr. Defaguliers did with sand. After condensing the air, and finding the excitation of it impossible, as usual, I let the air suddenly out, to see whether the tube would then show any effect of the preceding friction; but it had not acquired the smallest degree of electricity; though the first stroke of the rubber, immediately afterwards, made it give sparks to the finger at the distance of two and three inches. Perhaps the degree of moisture it had contracted was very slight, and expelled by the act of excitation.

UPON being desired to repeat this experiment with a particular view to the moisture; I observed that not the least cloudiness could be perceived to adhere to the glass, at the time that it was absolutely incapable of excitation. When one part of the tube was made warm, and the other left cold, the same stroke of the rubber would excite the warm part, without in the least affecting the rest. But still the cold part of the tube appeared not in the least more cloudy than that which was warm; and the moment the air was let out, the first stroke of the rubber made the whole strongly electrical.

WILLING

WILLING to ascertain whether the condensing of air necessarily introduced more moisture into a glass vessel than the air could hold in perfect solution, I constructed a glass condenser in such a manner, that I could charge and discharge small phials in the inside of it; concluding, that if the additional air brought more additional moisture, it would be impossible to charge a phial at all in those circumstances; whereas, if the air was free from moisture, it would make the phial hold a greater charge, double in two atmospheres, treble in three, &c. Accordingly, I charged a tube about three quarters of an inch in diameter, and coated about eight inches, in the glass vessel, containing about two atmospheres; and it received a much greater charge than it could be made to take in the open air, and as near as could be judged, by the report and flash, twice as great. At last the tube burst by a spontaneous discharge, after being charged and discharged three or four times, in the condensed air. It is not at all probable, that it could have been broke by any charge it could have held in the open air. This experiment seemed to determine, that there was no very great degree of moisture introduced into the glass vessel by the condensation of air.

I AFTERWARDS found that experiments on condensed air had been made by Mr. Du Fay and others, but not with all the circumstances above mentioned.

SOME of my electrical friends are of opinion, that the reason why a tube with condensed air in it cannot be excited is, that the dense air within prevents the electric fluid from being forced out of the inside of the tube, without which none can be forced into the outside; and that heating the tube makes the air within less electrical, and the tube also; in consequence of which, it may more easily part with the fluid on one side, and admits it on the other. But upon this principle how can a solid stick of glass be excited?

IMAGINING that a greater quantity of electric fire would be produced from the friction of larger globes than those of the usual size, I provided myself on the 24th of April 1766, with a globe seventeen inches and a half in diameter. It had only one neck, and was made exceedingly well; only being rather too large for the mouth of the furnace, a small coal had stuck to its equatorial diameter, which, when it was struck off, made a small hole in it. This, in some measure, disfigured the globe, but I never imagined it could prevent its excitation in any great degree; so that I still indulged hopes of acquiring, by its means, a prodigious power of electricity. But what was my surprize when, after I had got it mounted in the best manner possible, and after trying, for hours together, every method of friction, in the most favourable circumstances for excitation, I could scarce get the appearance of fire from it; the sparks from  
the

the prime conductor being barely visible.

ACQUAINTING Dr. Franklin with my disappointment, he advised me to get the first coat of the globe taken off with emery ; as it had often been observed, that many globes would not work well, till after a considerable time, when the glass-house coat, as it may be called, is worn off. This operation I accordingly performed upon it, and incredibly laborious it proved ; which greatly increased my disappointment, when I found that it had all been labour in vain, for the globe had no more electrical power than before.

DISPAIRING of making any thing of this globe, I laid it aside, and, on the 22d of May, got another, about fourteen inches in diameter. In blowing this globe, every circumstance that I could imagine had, in the least, contributed to my ill-success with the former, was carefully avoided. The former was made late in the week, when the metal had been long in fusion ; because I had been told, that globes made in that state of the metal were always the best for electrical purposes. This was blown early in the week, when the workmen say the metal is most transparent, and freest from all kinds of imperfections. The former was warmed, in the course of making it, in a place in which wood and coals were frequently thrown, to keep up the heat. This was kept free from the fumes of any fuel whatever. Nothing could be finer than the metal of this globe,

nothing more perfect in its form. It was also very well mounted, and I did not doubt of success. But, after all, this globe, if possible, gave less fire than the former. I had recourse to every method of excitation that I had ever heard of, or could myself imagine, but all in vain. The thing looked like enchantment.

WHILST I was thinking over every thing that I could imagine might possibly be the cause of my ill success with these globes, I recollected, that another globe, which I had got made for a friend, in the same state of the metal with my last, and only an inch and half less in diameter, acted exceedingly well; and that there was no other apparent difference between them, but that his had two necks, and an axis quite through it; whereas mine had only one neck, and no axis at all. Willing to try every thing, I resolved to get the brass cap of my globe perforated, and a small wire introduced, to serve instead of an axis. This was done; but, in making the perforation, it happened, unfortunately, as I then thought, but the most fortunately in the world as it proved, that a lump of hard cement, about the bigness of a small walnut was pushed into the inside of the globe. Vexatious as this circumstance was, I was impatient to try my new experiment, and immediately began to whirl the globe, with this succedaneum of an axis, though the cement was all the while rattling, and fouling the inside.

I HAD

I HAD not whirled the globe long, in these circumstances, before I plainly perceived that its power increased. After some time it was pretty considerable, and I did not doubt but it was owing to the axis; nay I had formed a pretty plausible theory, to account for an axis being necessary to a globe of such a size. Willing, however, to verify the fact, and ascertain my new hypothesis, I took out the wire; but, to my surprise, found the virtue of the globe not at all diminished. On the contrary, it continued increasing, and by the time that the cement was well broken, and dispersed, so as to have given a kind of lining to the globe, its power was exceedingly strong, and it acted as well as any globe I had ever seen. In this state, I observed, that after exciting any part of the surface, the small pieces of cement in the inside, to the distance of about two inches, would jump from the finger, or any conductor, presented on the outside.

HAVING, in this unexpected manner, made a perfect cure of this smaller globe, I remounted the larger, and considering, that the cement could probably act only as any other electric lining, I introduced into it some pounded sulphur, mixed with some flower of brimstone; and found that, as soon as there was enough to render it semi-opaque, it acted very well.

IN this state, the appearance of the globe was, in several respects, very remarkable. The part that was rubbed had none of the

fulphur upon it, except those places where the polish had been, in some measure, taken off by the emery, in the first operation. These being circular, the brimstone lay upon them, like the belts of Jupiter. The hemisphere opposite to the neck had twice as much sulphur upon it as the other; and, in both hemispheres, the sulphur lay thicker, as it receded from the equatorial diameter.

I AFTERWARDS put as much more sulphur into it, which doubled the lining equally every where; but left two or three great heaps, in particular parts of the equatorial diameter, where it was rubbed, and where I could perceive no defect of polish. Whirling the globe, upon this, I found the virtue almost quite gone, and even the amalgam could not revive it. Endeavouring to take the sulphur out of the globe, I broke a great hole into it; and also the new globe was broken the same day, by a lump of hard cement, in the inside, falling from the top to the bottom. These accidents rendered my experiments incomplete.

I THEN proposed to get another large globe, with one neck, and a large hole in the opposite side; by means of which I could easily put different substances into it, and take them out again, in order to find the cause of the appearances above mentioned. But apprehending this course of experiments might prove a little too expensive, and after all, terminate in nothing, I unwillingly desisted.

I SHALL



I SHALL add to this section, on the subject of excitation, that I once whirled a very thin globe, about six or seven inches in diameter, which was made to weigh air, and not one fourth part so thick as a common Florence flask. It was excited very powerfully by a piece of leather which had been soaked in a mixture of tallow and bees wax, and into which a quantity of amalgam had been worked. With this globe I could make my common jar discharge itself over more than five inches of the external surface, which I reckon to be a considerable proof of its power. It seems to follow from this experiment, that the thinness of glass globes, or tubes, is by no means any obstruction to their electric power.

IN the course of these experiments I had read Mr. Bergman's account of his curing a globe by a lining of melted sulphur, and had proposed to try that in the last place, on account of the disagreeable operation; but found it superseded in the manner described above.

## SECTION II.

EXPERIMENTS WHICH PROVE A CURRENT  
OF AIR FROM THE POINTS OF BODIES  
ELECTRIFIED EITHER POSITIVELY OR  
NEGATIVELY.

**D**URING a course of electrical experiments, made to divert some of my friends, one of the company happened to present a pointed wire to my hand, as I was standing upon an insulated stool; when I was surprised to perceive a cool blast proceeding from it; though, according to Dr. Franklin's theory, the current of the fluid went from my hand to the point. I then presented my nostrils to the point, and perceived the same strong phosphoreal smell, as if the point had been electrified positively. These facts made me entertain some doubts about the direction of the current, and the principles of Dr. Franklin's theory, and led me to the following course of experiments; which prove nothing against that theory, but establish a real current of air from the points of all electrified bodies.

CONSIDERING that flame is the least sensibly affected with electrical attraction or repulsion, but most easily with the least breath of air; and not doubting at that time, but that the  
current

current of air would be in the direction of the fluid, being, as it were, impelled by it; I presented the flame of a candle to a pointed wire, electrified negatively, as well as positively. The blast was so strong (in both cases alike) as to lay bare the greatest part of the wick, the flame being driven from the point; and sometimes a pretty large candle would be actually blown out by the blast. But, in all cases, the effect was the same whether the electric fluid issued out of the point, or entered it.

PLACING the flame between two points, one of which communicated with the prime conductor electrified positively, and the other with the floor, the flame was blown from that which communicated with the conductor upon the other, but not to so great a distance as if the other had been away. Changing the points, the effect was still the same, whether that which communicated with the conductor was the more sharp, or the more blunt of the two, the flame always receding from it.

REVERSING this experiment, and making one of the points communicate with the rubber, and the other with the floor, the flame was always blown from the former towards the latter. It was evident, however, that the point which communicated with the floor had a current of air blowing from it likewise; for it counteracted the other, and would, when brought near the flame, raise it almost perpendi-

pendicular, when it had been blown quite aside by the other.

PLACING the flame between two points, one of which communicated with the rubber, and the other with the conductor, it was equally affected by both, being always blown from the point which was nearest to it.

IT was very observable, that, notwithstanding the current of air from the points affected the flame so remarkably; yet a small portion of it, when it was brought very near the point, would be strongly attracted by it, at the same time that the greatest part of the flame was, by the current of air, blown the contrary way. This effect was always the same, whether the point was electrified positively or negatively; though, I fancied that the negative point attracted the flame more sensibly than the other.

AFTERWARDS I diversified this experiment in the following manner. I charged the inside of a small jar positively, then setting it upon a glass stand, in contact with a pointed wire, I placed the flame of a candle within an inch of the point, and touched the wire of the jar, with a brass rod which I held in my hand. At every touch the flame was blown strongly from the point. Sometimes it would be blown out; but another point being held opposite to it, would support the flame; and more strongly, if that point was joined with the rod with which I touched the wire of  
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the jar. Charging the jar negatively in the inside, all the effects were the very same. Discharging the jar through the points, with the flame in a right line between them, it was disturbed, but not blown to one side more than the other.

To take off all the effect of the electrical attraction and repulsion, and leave the current of air to act singly, I interposed pieces of brass wire communicating with the earth, between the points of the wire and the flame; and found the blast to be rather increased than diminished thereby.

HAVING communicated these experiments to Dr. Franklin, he advised me to try the force of this current upon *paper vanes*, such as he has described in his letters: for, with him, they seemed to turn one way or the other indifferently, just as they happened to set out. Accordingly I took a cork, and stuck into the sides of it thirteen vanes, each being half a card, well dried, and each proceeding from the center of the cork. Into the cork I stuck a needle, by which I suspended the whole on a magnet.

THESE vanes I held two or three inches from the point of a wire, communicating with the outside coating of the jar, placed upon an electric stand, in the manner described above; and observed, that whenever I took a spark from the wire communicating with the inside, the vanes were strongly blown upon, and made to turn, as if the current of air had flowed from the point; at the  
same

same time that, according to Dr. Franklin's theory, the electric fluid was entering it. If they were made to turn the contrary way, the current soon stopped them, and never failed to bring them back, and make them move as before.

WHEN wires communicating with the floor were placed between the vanes and the point, to take off all the electrical attraction and repulsion, the vanes still moved as briskly as ever.

WHEN the jar was charged pretty high, the motion might be made so swift, that the separate vanes could hardly be distinguished, as the whole set turned round.

I MOREOVER observed, that the vanes were turned very briskly, not only when held near the point, but also when held any where within the distance of six or seven inches from the sides of the wire, which I made sometimes of a considerable length. The stream would turn the vanes one way on one side of the wire, and the contrary way on the other; and being removed quickly to the different sides, the direction of their course might be changed several times, in the discharge of one small jar.

I MADE points to project two ways at the same time, and observed, that the stream was the same from both, and also when the points were made to project at right angles from one another. In this position of the wires, it was amusing to observe, that the vanes would move one way, when held near one of the wires ;

wires; and immediately turn about and move the contrary way, if removed near the other.

HITHERTO I had made my vanes of very dry paper, in order to make them less affected by electrical attraction and repulsion, that so the current of air might be the more indisputable; but Mr. Canton desiring me to try vanes that were conductors, I first dipped my paper vanes in water, and afterwards made a set of tinsel, or thin pieces of brass, of the same form with the other. These vanes, being conductors of electricity, promoted a freer current of the electric matter, and consequently, occasioning a greater motion to be given to the air, they whirled about with more rapidity than the former. When they were insulated, they were affected just as the dry paper vanes had been.

WITH these vanes, I diversified the experiment in a manner which showed the sameness of the current, notwithstanding the change of electricity, in a clearer manner than before. I insulated a jar, with a wire projecting from the coating, and held the tinsel vanes near the extremity of it. All the time the jar was charging, the vanes turned with great rapidity, as if by a blast from the point. Keeping the jar, the pointed wire, and the vanes in the same situation, the gradual discharge of the jar, made by now and then touching the wire which communicated with the inside, made the vanes still turn the same way, and, as far as could be perceived, with the same force.

To

To diversify this experiment, I placed a charged jar upon a stool which had glass feet, a pointed wire projecting from the coating, a quantity of brass dust before the point, and a brass chain communicating with the ground on the other side of the dust. In this situation every attempt to discharge the jar threw a considerable quantity of the dust from the point, being raised about seven or eight inches, and blown to a considerable distance. Removing the pointed wire from the coating of the jar, and connecting it with the chain, the same attempt to discharge it blew the dust upon the jar. Using two points, one at the jar, and the other at the chain, the dust was disturbed, and raised up, but not blown one way more than the other. Fine flour answered nearly as well.

LASTLY, I made the experiment of the current with vanes in the form of a smoke jack, which answered as well as the others. They were moved when held more than a foot above the point, and likewise at a considerable distance below it, when it was turned downwards.

AFTER these experiments, I read in Mr. Wilson's treatise on electricity, that the vanes would not turn in vacuo. This I tried, and found it to be true, and at the same time I found, they would not turn in a close receiver, not exhausted, where the air was confined, and had not a free circulation.

THE current of air from the points of bodies electrified *plus* or *minus*, is not more difficult



difficult to be accounted for on Dr. Franklin's hypothesis of positive and negative electricity, than any other case of electrical repulsion. The particles of the atmosphere, near the points of electrified bodies, having, by their means, be-  
 possessed of more or less than their natural share of the electric fluid, must, according to the rule above mentioned, retire to places where they can discharge or replenish themselves, as occasion may require. If it be asked why the particles of the atmosphere do not, in the same manner, recede from all the parts of the electrified body, as well as from the points; it is answered, that, as the pressure of the atmosphere will prevent a vacuum, and as electrical attraction and repulsion are most powerful at the points of bodies, on account of the easier entrance or exit of the fluid at the points (upon whatever principle that effect depends) the electrified atmosphere (whether negative or positive makes no difference) must fly off at the points preferably to any other places, and the weight of the atmosphere will force the air of the neighbouring places upon the flatter parts of the electrified conductor, notwithstanding the real endeavour it may have to recede from it.

## SECTION

## SECTION III.

## EXPERIMENTS ON FIXED AIR, AND CHARCOAL.

**D**R. FRANKLIN, to whom I had communicated some imperfect experiments on the electricity of noxious air, recollected them when he was this last summer at Pyrmont, where a large body of fixed air always lies upon the surface of the medicinal spring (for this air is evidently specifically heavier than common air, and does not easily mix with it) but not having a proper apparatus, and the company there making experiments inconvenient, he did nothing that was decisive; though, from the little that he had an opportunity of doing, he imagined that it was not a conductor: and I have since found that this supposition was just. A charged phial may be dipped into a body of fixed air, resting on the surface of a fermenting vat, without being discharged. If two equal phials, however, be equally charged at the same time, and one of them be plunged into the fixed air, and the other kept out of it; the latter will always retain the charge longer than the former, which will sometimes retain it but a very short time; owing, as I suppose, to the *moisture*, which is readily absorbed by the fixed air.

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I ALSO found *inflammable* air to be the same as common, or fixed air, with respect to the power of conducting electricity.

THESE experiments on fixed air, imperfect as they were, led, however, to a discovery, which may possibly throw some new light upon some of the most fundamental principles of electricity.

BEING at that time but little acquainted with the nature of air, imagining, that fixed air only was unfit for respiration, and knowing that air was most injured by burning *charcoal*, I thought of trying charcoal itself in substance. Accordingly, on May the 4th, 1766, I tried charcoal, in a variety of ways and states; and found it to be, what I had suspected, an excellent conductor of electricity.

PRESENTING a piece of charcoal to the prime conductor, together with my finger, or a piece of brass wire, I constantly observed, that the electric spark struck the charcoal before either of the other conductors, if it happened to be advanced ever so little before them. Having a very rough surface, the charcoal did not take a dense spark from the conductor, till it was made a little smooth, and brought within about half an inch; when, to all appearance, it did quite as well as any piece of metal, there being a constant stream of dense and white electric fire between the conductor and it. I tried the charcoal in every state of heat or cold, and found no alteration of its conducting power.

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I PLACED

I PLACED a great number of pieces of charcoal, not less than twelve or twenty, of various sizes, in a circuit, and discharged a common jar through them; when, to all appearance, the discharge was as perfect, as if so many pieces of metal had been placed in the same manner. Two of the pieces, about the middle of the circuit, I placed about an inch and a half from one another; but, upon the discharge, the spark passed the interval very full and strong. A piece of charcoal also made the discharge at the wire with one spark, but the report was not so loud as when the discharge was made with a piece of metal. It was observable, that a black gross smoke rose from between each of the pieces of charcoal, at the moment of the discharge; but the ignition was momentary, and the fire could not be perceived on the charcoal.

To make the experiment of the conducting power of charcoal in the most indisputable manner, I took a piece of baked wood, which I had often used for the purpose of insulation, being an excellent non-conductor, and putting it into a long glass tube, I thrust it into the fire, and converted it into charcoal. In this operation, a very great quantity of gross smoke rose from it, so that, seeming to part with more of its moisture, one would have expected it would have come out a better non-conductor; but, upon trial, its electric property was quite gone, and it was become a very good conductor.

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THE experiments above mentioned were first made with *wood charcoal*, of which I found pieces of very different degrees of conducting power; but the most perfect conductors I have found of this kind are some pieces of *pit charcoal*. These seem to be, in all respects, as perfect conductors as metals. They receive a strong bright spark from the prime conductor, though seldom at above an inch distance, on account of the roughness on their surface, which cannot be taken off; and in discharging a jar through them, or with them, no person can imagine any difference between them and metal, either in the colour of the electric spark, or the sound made by the explosion. When they are broken, they exhibit an appearance which very much resembles that of broken steel. There is however a great variety in the electrical properties of different pieces of this kind of charcoal; and for want of proper opportunity I have not yet succeeded in ascertaining, with sufficient certainty, the circumstances, in the preparation, &c. on which this variety depends.

I WOULD have preferred the examination of wood charcoal on many accounts; particularly, as the same substance is, in this case, converted from a perfect electric to a perfect conductor; and all the degrees of conducting power may be found in different specimens of it; whereas pit coal is itself a conductor, though an imperfect one: but not having any opportunity, I procured specimens of all

the varieties I could imagine in the same heap of pit charcoal, with respect to their nearness or distance from the surface, &c. but though I examined them with all the care and attention that I could apply, and in every method that I could think of, the differences were so exceedingly small, if any, that I could not fix upon any circumstance that I could depend upon for the cause of them.

EVEN common cinders from an open fire, of the kind of coals which we generally burn, I find to be very little inferior to charcoal; which is suffered to flame, but covered very close as soon as it is well burnt, and before any ashes are formed. Coals and cinders from a common fire, being a very commodious subject for experiments, I did not fail to make as many upon them as I could imagine would be of any use; except that I had no opportunity of trying a sufficient variety of coals. I took several out of the fire after they had done blazing, some of which I covered with ashes, some I quenched in water, and some I left to cool in the open air. I also reduced some of the coals to cinders in a glass vessel, without suffering them to flame; and I treated in the very same manner various pieces of oak, cut from the same plank; but when I examined them, I found their differences, with respect to their power of conducting electricity, very inconsiderable, if any. I thought the cinder of a coal which we call kennel, and which is remarkable for flaming much while it burns, to be  
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a better conductor than a cinder from a common coal: but the difference might be owing to its more uniform texture, and smoother surface. Charcoal made of coals which yield a strong sulphureous smell when they are burnt, and of which the charcoal itself is not quite divested, was, to all appearance, as good a conductor as that of the other kind, which is more esteemed.

IN this course of experiments I found myself much at a loss for a sufficiently accurate method of ascertaining the difference of conducting substances, and I wish that electricians would endeavour to find such a measure. One of the best that I am acquainted with, and which I applied among others on this occasion, is by the residuum of discharges, measured by Mr. Lane's electrometer. It is well known, that the worse the conductors are that form the circuit, the greater the residuum will be left in a jar after a discharge; and Mr. Lane's electrometer, which measures an explosion, will likewise measure the residuum. To apply this method with accuracy, I put pieces of charcoal, &c. of the same length into the circuit, I used the very same chain in every experiment, and the same disposition of every part of the apparatus; I also made the explosions exactly equal, and after every discharge completed the circuit by the chain before I took the residuum; and lastly, I was careful to take up the same time in each operation, which I repeated very often. This method of measuring the con-

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ducting power of substances I learned of Mr. Lane.

IN the prosecution of these experiments on charcoal, I burned a piece which I had found to be a most excellent conductor, first between two crucibles, and then in the open fire, and tried it at different times till it was almost burned away; but, contrary to my expectations, I found its property very little diminished. I was, likewise, surprised to find that *foot*, whether of wood coal, or pit coal, hardly conducted at all. I made five or six inches of the foot of pit coal part of the electric circuit, which completed the communication between the inside and outside of a charged jar for several seconds; and yet found the charge not much diminished. A piece of wood foot, which is a firm shining substance, which does not soil the fingers, and which seems to break in a polish in several places, would hardly conduct any part of a charge in the least sensible degree. When rubbed against my hand, or my waistcoat in frosty weather (though it was difficult to find any part of it that was large and smooth enough for the purpose) I more than once thought it attracted the thread of trial. The snuff of a candle would not conduct a shock, though it was placed in the middle of the circuit, and it was easily set on fire by the explosion of a small jar.

BUT notwithstanding my want of success, I make no doubt, but that any person of tolerable sagacity, who has an opportunity of making



ing experiments in a laboratory, where he could reduce to a coal all kinds substances, in every variety of method, might very soon ascertain what it is that makes charcoal a conductor of electricity. In all the methods in which I could make charcoal, the fume of the bodies was suffered to escape; but let trials be made of substances, reduced to a coal without any communication with the open air, or where the vapours emitted from them shall meet with different degrees of resistance to their escape, ascertained by actual pressure.

CHARCOAL, besides its property of conducting electricity, is, on many other accounts, a very remarkable substance; being indestructible by any method, besides burning in the open air; and yet it seems not to have been sufficiently studied by any chymist. A proper examination of it promises very fair, not only to ascertain the cause of its conducting, and, perhaps, of all conducting powers; but to be an opening to various other important discoveries in chymistry and Natural Philosophy; and the subject seems to be fairly within our reach.

PIT COAL, and probably all other substances, at the same time that they lose much of their weight, increase considerably in their bulk in the operation of charring. Does it not seem to follow from hence, that its conducting power may possibly be owing to the largeness of its pores, agreeable to the hypothesis of Dr. Franklin, that electric

substances have exceeding small pores, which dispose them to break with a polish.

OR, since the calces of metals, which are electric bodies, become metals, and conductors, by being fused in contact with charcoal; are not metals themselves conductors of electricity, in consequence of something they get from the charcoal?

THIS course of experiments, however, evidently overturns one of the earliest, and, hitherto, universally received maxims in electricity, viz. that *water* and *metals* are conductors, and all other bodies non-conductors: for we have here a substance, which is clearly neither water, nor a metal, and yet a good conductor.

N. B. I HAVE since found that it is the *degree of heat* with which charcoal is made, on which the *degree of its conducting power* depends. An account of this and other observations on charcoal may be seen in a paper of mine on the subject\*.

\* Phil. Transf. Vol. lx. p. 211.

## S E C T I O N IV.

EXPERIMENTS ON THE CONDUCTING  
POWER OF VARIOUS SUBSTANCES.

**F**INDING some contrariety of opinion among electricians about the nature of ice, some saying it was a conductor of electricity, and others a non-conductor, so as even to be capable of being charged like glais, I took the opportunity of a pretty severe frost, in the month of February, to assure myself of the fact.

IN order to this, I took a large piece of ice, washed it very clean, and scraped off all the sharp points about it. After this, when it was again perfectly frozen, I insulated it, at night, in the open air, whither I had carried my machine on purpose, at the same time that it was freezing intensely.

WHEN, by drawing a feather over its surface, I found it be perfectly dry, I electrified it, and fetched large sparks, not less than an inch in length, from all parts of it. I charged a jar at it, almost as well as at the prime conductor; I also discharged the jar through it, and along the surface of it, in several places; so that I had no doubt, but that ice was, nearly, as good a conductor of electricity as water. To try the same to more advantage, I  
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took a charged jar into the open fields; and, by means of a great length of chain, discharged it along a large surface of ice on a pond, whilst the surface was very dry, and the frost continued very intense. But the ice being not so good a conductor as metal, if the chain communicating with the outside of the jar happened to lie five or six inches from the knob of the wire communicating with the inside, the fire would strike to the chain, along the surface of the ice, without entering it.

SNOW is evidently not so good a conductor as ice; probably because its parts do not lie in contact with one another, as those of ice.

FINDING also that electricians were not perfectly agreed about the conducting power of *hot glass*, and that the methods which had been used to prove it were liable to objection; since, when the electricity was communicated along the outside of the glass, it might be said that the *hot air*, and not the hot glass was the conductor; it occurred to me, that the following experiment would determine this affair, in a more satisfactory manner than it had hitherto been done.

I PROCURED a glass tube, about four feet long; and, by means of mercury in the inside, and tinfoil on the outside, I charged about nine inches of the lower part of it. Then carefully slipping off the tinfoil, and pouring out the mercury, I heated the charged

ed part of the glass red-hot; and found, upon replacing the coating, that it was discharged.

I MADE the experiment a second time, with the same success; so that I had no doubt, but that glass, when red-hot, was pervious to the electric fluid. It could not have gone round from the inside to the outside, without going over a surface of six feet of glass, the greatest part of which was kept very cold, and all of it exceedingly dry.

THAT the charge had not been lost by changing the quicksilver was evident: for when I repeated that part of the experiment, without heating the glass, the charge was found to be very little diminished.

SOME time after, when I was preparing some baked wood for the purposes of insulation, I found, that if I used them soon after they were taken out of the oven, they would not answer my purpose at all. The electricity went off by them to the floor. But when they had stood, in the very same situation, till they were cold, they insulated very well.

UPON this, I made a piece of baked wood, which I had formerly used for insulation, pretty hot; and when it was so hot, that I could hardly hold it in my hand, it took a slender spark from the conductor, about an inch long; but it would not discharge a jar at once. It did it however silently, pretty much like moist wood.

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THE consideration of the conducting power of charcoal, and the manner in which it is made, namely, by burning inflammable substances, in a close place, and generally without flaming, led me to make a few experiments on the conducting power of the effluvia of flaming bodies, at the very time of their emission: for whatever those effluvia be, they seemed in some measure to contain the conducting principle.

THE conducting power of the flame of a candle was observed very early; but it was not compared with that of other things, and it had by some been supposed to be nothing more than the heat communicated to the neighbouring air. The experiments I am going to recite seem to overturn this hypothesis.

MARCH the 14th, a small charged phial held not longer than a second within two or three inches of the flame of a candle, either above or below it, where the heat was altogether inconsiderable, and the rarefaction of the air in a manner nothing, was totally discharged. The event was the same when I used the flame of a wax candle, or the flame of spirit of wine. When it was held much nearer to a red-hot poker, it was not discharged near so soon; and when it was held exceedingly near to a piece of red-hot glass, it was not discharged at all, except by one explosion, seemingly conducted by the hot glass. Similar experiments were made by placing

placing the candle, the poker, and the hot glass near the prime conductor. It was also found, that the small phial above mentioned could not be discharged in the focus of a concave mirror.

BUT the small jar above mentioned was discharged in these experiments silently; and though they seemed to be clearly in favour of the conducting power of the effluvia, which pass off in flame, there was nothing very striking in them; but afterwards, when I had constructed an electrical battery, I repeated the experiments in a much more striking and convincing manner.

DECEMBER the 15th, I brought the flame of a candle between two brass knobs, one communicating with the inside, and the other with the outside of the battery; and observed, that as the flame advanced towards them, it began to be put into a quivering motion, exceedingly quick, and was strongly drawn both ways towards each knob, leaving the wick bare at the top; and as soon as the flame was quite between the rods, the battery discharged at once, at the distance of three inches and an half. This is a very fine experiment. The interposition of the flame between the two brass rods is like putting fire to a train of gunpowder, which explodes immediately.

WHEN I advanced the ignited wick of a candle, just blown out, towards the rods, it was ventilated very briskly; and when it was put between them, when separated to about the

the distance of an inch, the discharge was made, and the candle blown in again.

To compare the conducting power of flame with that of other bodies, which had more heat but less effluvia, I put a red-hot poker between the two rods, but it did not promote the discharge of the battery till they were brought within about an inch and an half of one another; so that the explosion was made at about twice the usual distance, allowing for the space occupied by the poker itself; and yet the air in the neighbourhood of the poker was more than ten times hotter than in the neighbourhood of the candle, considering the distance at which they were held from the rods. Both sides of the hot poker were marked with an imperfect circle, like those that were impressed on each of the knobs; an account of which will be given hereafter.

I THEN interposed a piece of red-hot glass, which has as great a heat as the iron, but emits less effluvia; but it did not promote the discharge till the brass rods were brought within an inch of one another, which was so near, that the glass almost touched them both.

As I was diversifying the experiments concerning the passage of the electric explosion over the surfaces of various bodies, as will be mentioned hereafter, I accidentally discovered how exceedingly poor a conductor is *oil* of every kind; insomuch that I think it ought rather to be classed among electric substances; though



though before that time, I imagined that oil did not differ very much from water, with respect to it conducting power. I had been led into the mistake by some experiments of Mr. Wilson, who has somewhere advanced the proposition above mentioned; and argues that the tourmalin is possessed of a fixed kind of electricity, incapable of being conducted away, because it retains the separate power of each of its sides, though surrounded with melted grease; whereas I find, that nothing of an oily nature will conduct electricity.

LAYING a chain, which communicated with the outside of my battery, in a dish of melted tallow, I brought a brass rod communicating with the inside towards it, in order to make the discharge, by transmitting the explosion over the surface without entering it; when I was surprised to find, not only that the electric matter would not take the surface, but that, though it attracted a column of tallow at the distance of about three quarters of an inch (which was thicker in proportion as the rod was brought near the surface) and though I continued amusing myself with this column of tallow a considerable time; in which state it formed a complete communication between both sides of the battery, yet the charge was very little dissipated. I repeated this experiment, with the same event, with oil of olives, the thinnest oil of turpentine, and even ether. A plate of common oil of olives connected the inside and outside of the battery for near ten minutes, without my being able to

to perceive that the charge was more dissipated than it would have been without that communication. Ether is the slightest fluid in nature next to air ; yet, being properly an oil, it proved no better a conductor than the most tenacious. I was most surprised that the ether did not take fire by this treatment, as nothing is more inflammable ; and if the electric matter can pass through it, nothing fires so soon.

FROM these experiments, and those above mentioned, on ice, I concluded, that fluidity, as such, contributes nothing to the conducting power of substances, separate from the heat which makes them fluid. To complete my experiments on oils, I filled phials with all kinds of oils, according to their chymical distinctions, including the finest *essential oils*, the strongly *empyreumatic*, and those that are termed mineral, as *oil of amber* ; and found them all incapable of giving a shock. But I found that this method of trying the conducting power of substances, viz. by inclosing them in phials, and endeavouring to give shocks by them is very inaccurate, showing them to be better conductors than they really are. Pounded glass, flower of brimstone, and other electric substances gave a considerable shock ; but a bottle containing nothing but air gave a greater shock than any of them ; though the wire inserted into it was very blunt, and was kept in the center of the bottle. Finding, by these experiments, that oil plainly conducted much less than air, I  
endeavour-

endeavoured to charge a plate of oil like a plate of glass; and for this purpose I perforated a glass salver, and thereby gave a coating of tinfoil to both sides of a quantity of oil poured into it; but the brim of the salver would not contain enough to give it a sufficient thickness; otherwise, I make no doubt, but that a shock might be given by it better than by air.

I SHALL just mention upon this subject, what I lately observed, and do not know whether it has been noticed by any writer, that *ice of oil*, contrary to ice of water, is specifically heavier than the fluid substance, and sinks in it \*.

FINDING so great an agreement, with respect to electric properties, in this whole chymical class of bodies, I began a kind of course of chymical electricity; but had not leisure, or opportunity to pursue it as it deserved. The few hints that I collected may possibly be of service to future inquirers; and for this

\* OTHER persons, I find, have made experiments, which show how imperfect a conductor oil is. The following proofs of this are exceedingly curious and pleasing. Mr. Cigna observed electrical attraction and repulsion between conducting substances plunged in oil. Noller's Letters, Vol. iii. p. 168.

MONSIEUR VILLETTE, optician at Liege, filled a dish of metal with oil, and when he had electrified the dish, he plunged a needle into the oil, and received a very strong spark as soon as the point of it came within a small distance of the dish. A small cork ball being made to swim in this oil, upon the approach of the thicker end of the stalk of a lime, plunged to the bottom, and immediately rose again to the top. Noller's Letters, Vol. iii. p. 312.

reason I shall note them just as they occurred, though they contain little that is remarkable.

ALL *saline substances* that I examined proved, in general, pretty good conductors. I tried most of them by making the discharge of the battery through them when insulated; which appears to me to be a very good method, indeed the only one that can well be depended upon. In discharging the battery with a piece of *alum*, the explosion was attended with a peculiar hissing noise, like that of a squib. *Rock salt* conducted pretty well, but not quite so well as the *alum*. The electric spark upon it was peculiarly red. *Sal ammoniac* exceeded them both in its conducting powers, but it would not take the least sensible spark; so that it seemed made up of an infinite number of the finest points. *Volatile sal ammoniac* I only tried in a phial, when it gave a small shock. *Salt petre* did not conduct so well as *sal ammoniac*. Endeavouring to make the electric explosion pass over its surface, it was dispersed into a great number of fragments in all directions with considerable violence. *Selenitic salt* conducted a shock but poorly. *Vitriolated tartar* gave a small shock. *White Sugar* seems to be an exception to this rule: for it may be fairly said to be no conductor; as the charge of the battery would hardly pass through it in the least degree.

THE *metallic salts* in general conducted better than other neutrals: *blue and green vitriol*

*trial* conducted very well, though they would not transmit a shock.

THAT *ores* in which the metal is really in a metalline state should be very good conductors might naturally be expected. Thus a piece of gold ore from Mexico was hardly to be distinguished, in this respect, from the metal itself; and a piece of silver ore from Potosi, though mixed with pyrites conducted very well. But even ores in which the metal is mineralized with sulphur and arsenic, as the ores of lead and tin, and *cinnabar* the ore of quicksilver were little, if at all, inferior to them. The *cinnabar* that I tried was factitious; but there can be no doubt of its being the same as the native. When I made the explosion of the battery pass through it, it was rent into many pieces, and the fragments dispersed in all directions. Ores, however, that contain nothing but the earth of the metal conduct electricity but little better than other stones; though I thought that all the specimens of iron ore that I tried conducted better than marble \*.

I EXAMINED some *black sand* that came from the coast of Africa, which is a good iron, and part of which is affected by the magnet as much as steel filings; and found it to conduct electricity, but not a shock. Separating with a magnet all that would be easily

\* I FIND that Mr. Boze, very early, thought it was easy to distinguish the ores of metals from other earthy substances, by means of their greater conducting power. Dantzick Memoirs, Vol. i. p. 293.

attracted by it (about one sixth of the whole) it conducted a shock very well. The rest would hardly conduct at all.

THOUGH I think I may venture to say, that the true and proper ores of the more valuable metals might be known by their property of conducting electricity, I cannot say that electricity will furnish any rule to ascertain the value of the different ores of the same metal. I tried two pieces of copper ore, one the most valuable that is known, and another of only about half the value; but they were hardly to be distinguished from one another in their conducting power.

BLACK *lead* in a pencil conducted a shock seemingly like metal or charcoal. A small lump of it took as full and strong a spark from the prime conductor as a brass knob.

ALL the *stony substances* that I tried conducted very well, though dry and warm. Even a piece of polished *agate*, though semipellucid, received the electric spark into its substance; though it would pass over about three quarters of an inch of its surface to reach the finger that held it, and it discharged the battery but slowly. *Limestone*, and *lime* just burnt were equally imperfect conductors, hardly to be distinguished from one another. *Lapis hæmatites*, and *touchstone* both conducted pretty well; as did a piece of *gypsum*, and *plaster of Paris*, only the latter, having a smoother surface, took a stronger spark. A piece of *slate*, such as is commonly used to write on, was a much better conductor

ductor than a piece of *free stone*, which conducted very poorly. *Marbles* also conducted considerably better than free stone. I found very little difference among any of the specimens of marble that I tried, in which was a piece of Egyptian granite. A piece of Spanish chalk, which is a *talc*, conducted pretty much like marble.

A LARGE piece of white *spar*, with a tinge of blue, and semi-transparent, would hardly conduct in the least degree. I took pretty strong sparks from the prime conductor while it was in contact with it.

A PIECE of *pyrites* of a black colour took sparks at a considerable distance from the prime conductor, like some of the inferior pieces of charcoal. Another piece of pyrites, which had been part of a regular sphere, consisting of a shining metallic matter, did not conduct near so well, though much better than any other stony substance. It was a kind of medium between a stone and an ore.

A PIECE of *asbestos* from Scotland, just as it is taken from its bed, would not conduct. It was in contact with the conductor, while I took sparks at the distance of half an inch with a moderate electrification.

OF *liquid substances*, *oil of vitriol* conducted pretty well, and the most highly rectified *spirit of wine* gave a shock much like water, but perhaps not quite so well.

THIS course of experiments on the conducting power of substances, according to  
P 3 their

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their chymical classes, would, probably, be very useful, if pursued with care. Those mentioned above were generally single experiments, which are not so much to be depended upon.

THERE are some other *mixed substances* whose conducting power I have tried, and because I think it would not be easy to say, *a priori*, to which of the two classes they belong, I shall just mention the result of my experiments upon them, nearly in the order in which they were made.

DRY *glue*, which is an animal substance, is a conductor of electricity, but does not conduct a shock.

POUNDED *glass* mixed with the white of an egg, and which had stood till it was perfectly dry, was a conductor. I had put it upon some broken jars, thinking that the composition would be an electric substance, and that it would make the jars hold a charge again.

PAINT, made of white lead and oil, very old, and dry, proved a conductor. I tried it in a china vessel which had been firmly pieced with it. A part of the vessel, through which there was no crack, would receive a charge very well; but a piece in which there was a crack, and which had been filled with this cement, could not be charged at all.

## SECTION



## SECTION V.

EXPERIMENTS ON THE DIFFUSION OF  
ELECTRICITY OVER THE SURFACES OF  
GLASS TUBES, CONTAINING A NEW ME-  
THOD OF GIVING THE ELECTRIC SHOCK.

**I**T had been observed by many electricians, that new globes are often difficult to excite; but I have made some experiments, which prove this fact, and other differences between new and old glass, in a more distinct manner than any thing else I have yet met with; but they leave the cause still unexplained.

THE most remarkable property of new flint glass is the easy diffusion of electricity over its surface. I have several times got tubes made two or three yards long, terminating in solid rods. These I have taken almost warm from the furnace, in the finest weather possible, and having immediately insulated them, and hanging pith balls at one extremity, have always found, that they would separate the moment that the wire of a charged phial was applied to the other end. This I had reason to think would be the case at almost any distance at which the experiment could be made. I have even charged a phial very sensibly, when it was held close to the glass, at the

distance of a yard from the wire of a charged phial, held close to another part of it, the coatings of both phials being held in my hands. When the same tubes were a few months older, I found that the electric virtue could not be diffused along their surfaces farther than about half a yard.

SOME tubes, which I have tried the day they were made, I have found impossible to be excited in the least degree, even with the use of oiled silk and amalgam, for an hour together; when a single stroke of the same rubber has rendered other tubes highly electrical, and two or three have made them to emit spontaneous pencils. The same new tubes, upon being much rubbed, have begun to be excited, and in a few days have acted pretty well.

BUT that the first coat of new glass is, in some measure, a conductor of electricity, was most evident from some experiments which I made with long and very thin tubes, which were blown some time in the month of March. These, to amuse myself, I coated in different places, and the diffusion of electricity, from the coated part to that which was not coated, appeared to me very extraordinary. I think my reader will not be displeased if I relate a few of the particulars.

I PROCURED a tube, open at both ends, about a yard in length, but of very unequal width. About three inches of the middle part of it I coated on both sides; and charging it, by means of a wire introduced at one of the ends;

ends; I perceived, not only that the part through which I had introduced the wire was strongly electrical on the outside, but that at the opposite end, where there was neither coating nor wire, the fire crackled under my fingers, as I drew the tube through them, and a flame seemed to issue continually out at both the ends, while it was at rest and charged. N. B. One end of this tube was broken, and rough, the other was smooth.

I PROCURED another tube, about an inch in diameter, and very thin. It was about three feet and a half in length, and closed at one end. About nine inches below the mouth, I coated three inches of it, both on the inside and outside. This part I charged, and then observed the whole tube, to the very extremity of it, to be strongly electrical, crackling very loud when I drew my hand along it, and giving sparks, as from an excited tube, at about the distance of an inch, all the way.

To give the reader a better idea of these experiments, I have given a drawing [Pl. I. fig. 7] of one of the tubes with which they were made. It is open at one end, and the part [a] is coated.

AFTER drawing the whole tube through my hand, all the electricity on the outside was discharged; but, upon putting my finger within the mouth of the tube, an effort to discharge itself seemed to be produced, which showed itself by a light streaming visibly from the coating, both towards the finger and

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and likewise as vigorously towards the opposite end of the tube. After this I found all the outside of the tube loaded with electricity as before, which might be taken off, and revived again many times, with the same original charge; only it was weaker every time.

HOLDING this tube by the coated part, and presenting the uncoated outside, near the close end of the tube, to the prime conductor, the inside became charged as well as the outside; and, upon introducing a wire, a considerable explosion was made.

THE discharge made the outside strongly electrical, and by taking this electricity off, the tube was charged again very sensibly.

HOLDING it by the uncoated part, and presenting the coated part to the conductor, the inside became charged as before.

HAVING first perfectly discharged this tube, I closed the open end with cement, made of bees wax and turpentine, an inch or more in thickness; but still, by applying the outside of the tube (either the coated or the uncoated part) to the conductor, I found it manifestly charged, but not quite so high as when the end was left open, though the difference was not great.

I PROVIDED myself with another tube, about an inch and a quarter wide, and three feet long; but it was drawn out one foot more very small; and another foot at the extremity was solid, so that it was in all five feet long, I coated about four inches of this tube,

tube, two feet below the mouth of it. The balls being hung at the extremity of this tube, or rather of the solid rod in which it terminated, they separated the moment I began to charge the coated part. The discharge brought them together, though not immediately, but a second discharge would generally do it.

THE residuums of any of these tubes, of which so small a part was coated, were very considerable. I thought that all of them might be equal to the first discharge. In the last mentioned tube, there was a residuum after a great number of discharges, I believe twenty or thirty.

IMAGINING that the diffusion on the surfaces of the tubes above mentioned depended upon the *newness* of the glass, I preserved them six or seven months; having observed by examining them at proper intervals in the mean time, that this property, and others depending upon it, gradually lessened; and before this time it was quite gone. There was no diffusion of electricity over their surfaces, and they were as easily excited as other tubes, at the same time that they received a very good charge.

AT length, by some accident or other, all the tubes on which I had made these experiments were broken, except one, which was closed at one end, and which indeed, was the most remarkable of them all. Upon this tube, in the month of November, I began to renew my experiments, comparing it with others

tohers which I got made at that time, in order to ascertain on what circumstances this diffusion of electricity depended. These I shall distinctly relate, noting the time when each experiment was made, and every other circumstance which I can imagine could possibly have any influence in the case.

NOVEMBER the 13th, I once more endeavoured to repeat the experiments above mentioned with the old thin tube, with as much care and precaution as possible, but without the least success. At the same time I charged two other thin tubes, one closed, and the other open, after they had been made about six weeks, but without being used in the mean time, and they answered exactly as the former tube had done, when it was new. The charge from a small coated part diffused itself all over the tube; so that at the distance of a yard from the coating, it gave sparks to the finger of an inch in length, and in all respects exhibited the appearance of a tube fresh excited. On this occasion I first observed, what afterwards drew my attention in a more particular manner, that when my finger was brought to the tube about two inches above the coating [as at *b*. Pl. I. fig. 7.] it discharged a great quantity of that diffused electricity; and my whole arm was violently shocked.

NOVEMBER the 19th, After heating the old tube, and endeavouring to repeat the former experiments, both while very warm, and after it was cold again, but to as little purpose as before; I took it to the glass house,  
and

and got it made red-hot all over, so that it would easily bend any way; and as soon as it was cold, I tried the old experiments, and found that it had completely recovered its former property. Charging a small coated part, the electricity was diffused to the end of the tube, over three feet of dry glass, and it gave sparks at the distance of an inch in any part of it, exactly as if it had been excited with the best rubber. When it was drawn through my hand, whereby that diffusion was taken off, it presently returned again; and the extremity of the tube would get loaded while its communication with the coating had been cut off, by my hand being constantly held on the middle of it.

I ALSO observed, that the middle part of this tube, which had been oftenest heated, in melting the whole over again, one half at a time, had a much stronger diffusion than the other parts. It was no sooner taken off, than it appeared again, so that it gave a continual stream of fire.

THE quantity of *residuum* after a discharge of this tube was prodigious, so that the outside coating would, immediately after, give almost a constant stream of fire to any conductor presented to it, for a considerable time.

THIS tube was now, as it had been at the first, absolutely incapable of being excited with the best rubber.

JANUARY the 6th, 1767. Examining all the tubes with which I had made the experiments

ments of the diffusion, I found that property either quite, or very nearly gone. One of them I restored by heating it red-hot. Another I heated only at the end most remote from the coating; but there was no diffusion upon it, when the coated part was charged; the part which had not been made red-hot intercepting it.

NOVEMBER the 24th. In order to determine whether this property of diffusion depended in any measure upon the smoothness of the surface, I made a circular part of one of the thin tubes, about half a yard beyond the coated part, quite rough with emery, about three inches in length; but this did not prevent the diffusion in the least; both that rough part, and the smooth glass beyond it were as much loaded with the electricity as the rest.

I THEN took the polish off a line the whole length of the tube, from the coating to the extremity of it; but still the effect was the same: and I make no doubt would have been so if I had made all the surface rough.

IN order to ascertain whether this property depended upon the thinness of the tubes, I got one made of a twelfth of an inch thick, and used it immediately; the diffusion was very sensible, and it was incapable of being excited. This, however, was not always the case with tubes of so great a thickness.

NOVEMBER the 25th. Willing to carry this experiment a little farther, I got another tube four feet long, and of the eighth of an inch



inch thick. I coated a small part of it in the same manner as I had done the others, namely about three inches, at the distance of nine inches below the orifice, and observed the diffusion to be very remarkable in proportion to the charge it received, which was very moderate. It could not be sensibly excited in the least degree; except that, in the dark, an exceedingly small light was visible near the finger, when it touched any part of it, immediately after excitation; but not the least snapping could be perceived, nor any thing felt with the finger.

To find whether this property depended upon the *kind*, as well as the newness of the glass, I afterwards, coated a part of a very thin glass of the common bottle metal, but I found no diffusion upon it at all. It was what is commonly called a finging glass. I would have pursued this experiment by trying the same glass in other forms, and by trying other kinds of glass, but I had no opportunity.

I OBSERVED, in all the tubes which had the diffusion, that in drawing my hand from the extremity of them towards the coating, after they were charged, so as to take off the diffusion, there was a considerable noise at the orifice, as if the tube had been gradually discharging itself, and this operation did apparently lessen the charge.

IN the dark the electric fire seemed to pour perpetually from the open end, or both the ends, if they were both open; and when-  
ever

ever I drew my hand over it, the fire streamed from the coating towards my hand in a very beautiful manner.

IT was very remarkable, that, the first time I charged any of these tubes after they had stood a while, the diffusion was the most considerable, and that it lessened every successive charge; till, at last, it was exceedingly small; but after the tube had stood a few hours uncharged, it was as vigorous as ever.

DECEMBER the 1st. I, for the first time, took particular notice in charging a thin tube, and afterwards holding the coating in one hand, and drawing my other hand, so as to grasp the tube; beginning at that end which was most remote from the coating; that sometimes, when my hand came near the coating [as at *b*. Pl. I. fig. 7.] I received a very considerable shock through both my arms and in my breast, exactly like what is felt from the Leyden phial.

THE same day, I felt a similar shock from another thin tube; and what was more remarkable, I did not receive it till the third time of drawing my hand over the tube, having missed the stroke the two first times; though I moved my hand, as near as I could judge, in the same manner. This shock was not very great, but sensible in both arms.

DECEMBER the 3d. I received another shock, the third time of drawing my hand over the tube, and much more violent than the

the last; it affecting both my arms and breast. At this time I observed very exactly, that my hand was near two inches and an half from the coating, and that a strong light was visible under my hand, and extended to the coating. The diffusion at this time had not been very great, and the tube seemed to be about half discharged after the shock.

At that time I could not think of any plausible theory to account for this shock; but presently after I accidentally received another shock, in some respects similar to this, the theory of which I have been so happy as to investigate, and which may throw some light upon this.

DECEMBER the 21st. I made a Torricellian vacuum in a tube about a yard in length [Pl. I. fig. 8.] and holding one end of it in my hand, I presented a part near the other end to the prime conductor; and observed, that, while the electric fire was pouring along the whole length of it, I felt some peculiarly smart twitches every now and then in my hand, just such as are felt when a thin uncoated phial is held in the hand, while it is charged at the prime conductor, but more pungent. On removing the tube from the prime conductor, it threw out spontaneous sparks from the place where it had touched the conductor, exactly like those which issue from the wire of an over-charged phial; but they were longer, and much more beautiful. Then, bringing my other hand near the place where the tube had touched the conductor, I

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received

received a very considerable shock in both my arms and breast, exactly like that which I had received before from the thin tubes ; and, as with them, the shock was rather stronger in the hand which was brought to the tube than in that which held it. If, without bringing my other hand to the tube, I only presented it to the table, or any other conductor, it would throw out from the same place several strong sparks, attended with a flash of light, which filled the whole length of the tube. These sparks resembled those which issue from the wire of a charged phial, when it is presented to the like imperfect conductors, and at the same time held in the hand.

I AFTERWARDS observed, that the strongest shock which this tube could give was felt when one hand continued in the place where it held the tube, in order to charge it, and the other was made to touch the tube, an inch or two above it [as at *c.* Pl. I. fig. 8.] and at the instant of the stroke a very dense spark of electric fire was seen darting the whole length of the tube. When three persons besides myself joined hands, it shook all our arms greatly.

THE tube could not be discharged by putting one hand so near the other, unless that part of the tube had been brought to the conductor in charging it ; and if any particular part of the tube only, had been brought to the conductor, the discharge could not be made without touching that part.

WHEN the tube had given a shock from any one place, it would give one or two more smaller shocks from other places.

THE experiments I made with this tube being certain and invariable, and the shock I received from the other tubes precarious, I gave more particular attention to this, in order to ascertain the nature of this shock; thinking that, if I could accomplish this, it would assist me in the investigation of the other. Accordingly, I coated about six inches, near each end of the tube, [*a* and *b* Pl. I. fig. 8.] leaving the space of about half a yard of uncoated glass between the coatings; and observed, that when I held one of these coatings in my hand, and presented the other to the prime conductor, it always received a considerable charge, and was discharged in one bright spark at the distance of above an inch, and sometimes two inches, if, besides the coated part, I had likewise presented the uncoated part to the prime conductor; and sometimes the uncoated part would discharge itself by a bright flash to the lower coating, leaving the coated part charged as before. If I held the tube by the middle, where there was no coating, and presented one of the coatings to the conductor, it received a pretty good charge.

I THEN stood upon an insulated stool, and presenting one of the coatings to be charged, while I held the other; I observed, that it received not more than one fourth part of the

Q 2 charge

charge it had before ; upon which I immediately concluded, that the lower coating must have been charged negatively, whilst the upper was charged positively. This was quite confirmed by observing, that sparks could be drawn from my body, while I stood upon the stool presenting the tube to be charged, but no longer than till the tube had received its full charge ; and that then the explosion was as great as it had been when I stood upon the floor.

WHEN I insulated the tube, by placing it in a glass vessel, it was still less capable of taking a charge than when I stood upon a stool and held it, this method making a more perfect insulation. If any conductor was presented to the lower coating while the other was held to the prime conductor, sparks issued from it very plentifully till it had got a considerable charge ; when those sparks intirely ceased, and the tube, upon trial, gave a very great explosion.

THESE experiments make the theory of this new method of giving the electric shock pretty obvious. The electric matter thrown upon the upper coating repels an equal quantity from the inside of the tube opposite to it ; which, passing freely through the vacuum (as is visible in the dark), is accumulated on the inside of the other extremity of the tube, and thereby repels a quantity from the lower coating : so that the two coatings being in opposite states, though on the same  
side

side of the tube, the same kind of shock is given by them, as if they had been on opposite sides.

BEING fully satisfied with the experiments made by these two coatings, and the theory of them, I amused myself with coating the middle part of the tube in various ways.

WHEN the three coatings were about the same size, and placed at equal intervals; which ever of them was held in the hand, the other two were charged and discharged separately. If the coating of one of the ends was held in the hand, and the other two were charged, the greatest explosion was from that which was discharged first. If those two coatings were placed near one another, they were both discharged by the attempt to discharge either of them, and a flash of light was seen betwixt them both. In this case the explosion was sometimes made at the distance of two inches and a half.

WHEN the middle coating was made very large, and placed contiguous to the upper, the explosion was less; a spontaneous discharge being soon made to the lower coating.

WHEN the middle coating was taken away, it often happened that, in drawing the whole tube over the prime conductor, beginning at the upper coating; when it came to the lower, by which I held it, a spark would dart to it from all the uncoated part of the tube, which discharged the electricity of that part,

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while

while the upper coating still retained its proper charge.

WHEN this spontaneous discharge was not made, the explosion might be made at twice, once at the naked glass, near the lower coating, and again at the upper coating. If the discharge was first made at the upper coating, there remained very little for the lower part of the tube. And if the explosion was made about the middle of the tube, the whole was discharged at once, and in a very beautiful manner.

I MUST leave my reader to compare the theory of this shock with that given by the long and open tubes, as I am not able to do it to my entire satisfaction without more experiments; which, as I observed, are precarious, and which I had not leisure to attend to.

As this course of experiments was begun by an accidental observation of the different electric properties of new and old glass, I shall (after this long excursion, which I little foresaw) conclude with an experiment or two relating more immediately to the original subject of them.

IMAGINING that the difference between new and old glass might be owing to the larger superficial pores of the former, which made it approach to the nature of a conductor, and which contracted with time; I thought it might possibly be determined by the experiment of the metallic tinge, the wider pores receiving it better than the smaller;  
and



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and I was not disappointed in my expectations. November the 19th, I several times laid two glass tubes, one a very old one, and the other quite new, close together, with a piece of leaf gold or copper between them; and though I varied the disposition of them in every way that I could think of, and changed the tubes for others; I always found the new glass to receive a much fairer, more beautiful, and indelible impression than the old glass. Twice the quantity of the metal was in all the cases struck into it.

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SECTION

## SECTION VI.

EXPERIMENTS TO VERIFY SEVERAL PARTICULARS OF SIGNIOR BECCARIA'S THEORY OF ELECTRICITY; PARTICULARLY CONCERNING THE ELECTRIC MATTER CARRYING INTO ITS PATH LIGHT SUBSTANCES TO ASSIST ITS PASSAGE.

**B**EING greatly struck with Signior Beccaria's theory concerning the passage of the electric matter from the earth to the clouds, previous to a thunder storm, and thinking his experiments to prove the power of electricity to conduct into its path light substances that could assist its passage not quite satisfactory, I endeavoured to ascertain the fact in a better manner, and shall lay before my readers the result of my experiments.

NOVEMBER the 9th. I discharged frequent shocks, both of a common jar, and another of three square feet, through trains of brass dust, laid on a stool of baked wood, making interruptions in various parts of the train; and always found the brass dust scattered in the intervals, so as to connect the two disjointed ends of the train; but then it was likewise scattered nearly as much from almost all other parts of the train, and in all directions.

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The scattering from the train itself was probably occasioned by small electric sparks between the particles of the dust; which, causing a vacuum in the air, drove all that light matter to a considerable distance. But the particles of the dust which were strowed in the intervals of the train, some of which were, at least, three inches, could hardly be conveyed in that manner.

WHEN small trains were laid, the dispersion was the most considerable, and a light was very visible in the dark, illuminating the whole circuit. It made no difference, in any of these experiments, which way the shock was discharged.

WHEN I laid a considerable quantity of the dust at the ends of two pieces of chain, through which the shock passed, at the distance of about three inches from one another, the dust was always dispersed over the whole interval, but chiefly laterally; so that the greatest quantity of it lay in arches, extending both ways, and leaving very little of it in the middle of the path. It is probable, that the electric power would have spread it equably, but that the vacuum made in the air, by the passage of the fluid from one heap of dust to the other, dispersed it from the middle part.

I THEN insulated a jar of three square feet, and upon an adjoining glass stand laid a heap of brass dust; and at the distance of seven or eight inches a brass rod communicating with the outside of the jar. Upon bringing another  
other

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other rod, communicating with the inside, upon the heap of dust, it was dispersed in a beautiful manner, but not one way more than another. However, it presently reached the rod communicating with the outside.

MAKING two heaps, about eight inches asunder, I brought one rod communicating with the inside upon one of them, and another rod communicating with the outside upon the other. Both the heaps were dispersed in all directions, and soon met; presently after which the jar was discharged, by means of this dispersed dust, in one full explosion. When the two heaps were too far asunder to promote a full discharge at once, a gradual discharge was made through the scattered particles of the dust.

WHEN one heap of dust was laid in the center of the stand, and the two rods were made to approach on each side of it, they each attracted the dust from the side of the heap next to them, and repelled it again in all directions. When they came very near the heap, the discharge was made through it, without giving it any particular motion.

ALL these experiments show that light bodies, possessed of a considerable share of electricity, disperse in all directions, carrying the electric matter to places not abounding with it; and that they sometimes promote a sudden discharge of great quantities of that matter from places where it was lodged, to places where there was a defect of it. But  
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an accident led me to a much more beautiful, and perhaps a more satisfactory manner of demonstrating the last part of this proposition, than any that I hit upon while I was pursuing my experiments with that design.

DECEMBER the 11th. Hanging a drop of water upon the knob of a brass rod communicating with the inside of my battery, in order to observe what variety it might occasion in the circular spots, which will be mentioned hereafter ; I was greatly surprised to find the explosion made all at once, at the distance of two inches.

I, AFTERWARDS, put some brass dust upon a plate of metal communicating with the inside of the battery, and making the discharge through the dust, it exploded at the distance of an inch and a half. The dust rose towards the discharge rod, and from thence was dispersed in all directions.

THESE experiments are the more remarkable, as they demonstrate so great a difference between the distance at which the battery may be made to discharge at once, by the help of these light bodies, and without them. When the discharge of a battery by the knobs of brass rods, in the open air, is at the distance of about half an inch ; it will, by this means, be made at about two inches.

UNLESS a person try the following experiment, he will hardly conceive the extreme probability of the clouds and the rain being possessed of an electric virtue, in order to  
their

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their uniform dispersion, according to Signior Beccaria's theory. Put a quantity of brass dust into a coated jar, and when it is charged, invert it, and throw some of the dust out. It is very pleasing to see with what exact uniformity it will be spread over any flat surface, and fall just like rain or snow. In no other method can it be spread so equably.

It is taken for granted by Signior Beccaria and others, that persons are sometimes killed by lightning without being really touched by it, a vacuum of air only being suddenly made near them, and the air rushing out of their lungs to supply it; and with so much violence, that they could never recover their breath. As a proof of this, he says, that the lungs of such persons are found flaccid; whereas, when they are properly killed by the electric shock, the lungs are found inflated. This account always appeared to me highly improbable. It determined me, however, to make a few experiments, in order, if possible, to ascertain the fact with some degree of exactness. The result was as follows.

DECEMBER the 18th. I placed that part of an egg shell in which is a bladder of air within an inch of the place where I made the explosion of the battery, on the surface of some quicksilver; when the bladder was instantly burst, and the greatest part of it torn quite away. The shell was quite dry;  
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so that the bladder could not stretch in the least.

It is evident from this experiment, that there is a sensible expansion of the neighbouring air, to fill a vacuum made by the electric explosion; but that this is so considerable as to occasion the suffocation and death of any animal, is, I think, very improbable from the following facts.

I PUT a cork as slightly as possible in the mouth of a small phial; but, though I held it exceedingly near the place of the explosion, it was not drawn out.

I MADE the explosion pass over the surface of a moist bladder, stretched on the mouth of a galley-pot; but it produced no sensible effect upon it. I also held at one time the bill of a robin red-breast, and at another time the nose of a mouse near the electric explosion, but they did not seem to be at all affected by it. In order to examine the state of the lungs, I killed small animals by shocks discharged both through the brain, and through the lungs; but when they were dissected there appeared no difference. The lungs were in the very same state as when they were killed in another manner.

To these miscellaneous experiments, intended to verify several particulars of Signior Beccaria's theory of electricity, I shall add a small set, which, though they were begun before I had seen the author, are in some respects similar to his curious experiment

## 238 EXPERIMENTS RELATING TO

ment of discharging a plate of glass hanging by a silken string without giving motion to it; his being designed to ascertain the effect of the discharge upon the glass, and mine respecting the conducting substances that formed the circuit.

OCTOBER the 7th. To determine, if possible, the direction of the electric fluid in an electric explosion, I hung several brass balls by silken strings, and discharged shocks through them, when they were as much at rest as I could make them; but I could not perceive that any motion was given to them by the stroke. Afterwards, I discharged a jar a great number of times through small globules of quicksilver, laid on a smooth piece of glass; but could not perceive that they were driven one way more than another, though they were often thrown into disorder; probably by the repulsion of the air, occasioned by the vacuum of the explosion.

I THEN placed four cork or pith balls, at equal distances, upon a stool of baked wood, with a piece of chain at the same distance from the outermost balls; and observed, that, upon every attempt to make a discharge, the two middle balls were driven close together, while the two outermost were each of them attracted to the piece of chain that was next to it. Then, laying a great number of bits of threads in the same manner upon the stool, several of the pieces that lay near the chain stuck



stuck to them, and a great number of those that lay in the middle were driven together in a heap.

THE attraction to the chains I attribute to the electricity given to them by their connection with the jar, which would be greatly increased by the attempt to make an explosion; and the crowding together of the pieces in the middle of the circuit, I attribute to the current of air blowing them together from both the extremities of the chain. Thus part of the flame of a candle next to an electrified point will be attracted by the power of electricity, while the rest of the flame will be repelled from it by the current of air.

THESE experiments led me to make a discharge through an insulated bell, in order to observe in what manner it would be affected by the electric shock only, when it was not touched by any thing else. Accordingly, I made the discharge of the battery through it several times; and by each explosion it was made to ring, as loud as it could be made to do with a pretty smart stroke of one's finger nail.

I ALSO made a discharge of the battery through the external coating of a glass jar, but without touching it with the discharging rods; and it plainly produced the same tone, as when it was rung by percussion.

## SECTION

## SECTION VII.

VARIOUS EXPERIMENTS RELATING TO  
CHARGING AND DISCHARGING GLASS  
JARS AND BATTERIES.

**A**S several things have occurred, in the course of my experiments, relating to charging and discharging both common jars and large electrical batteries, which I have not seen in the writings of any electricians; and as some of the facts are not easily accounted for, I shall mention a few of the more remarkable of them, just as they happened.

APRIL the 28th. As I was amusing myself with charging three jars of the ordinary size, while they stood upon a metal plate on the table, with their wires at different distances from the same prime conductor, which was fixed on pillars of baked wood; I observed, that whenever one of the jars, which stood next to the conductor, discharged itself, the others would discharge themselves too; though they were far from having received their full charge, being placed at a greater distance from the common conductor, and consequently having taken but few sparks, in comparison of that which stood the nearest.

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A VARIETY of experiments seem to show, that, while a jar continues charged, the electric matter is continually insinuating itself farther and farther into the substance of the glass, so that the hazard of its bursting is the greatest some time after the charging is over.

MAY the 26th. After having charged forty-one jars together, each containing about a square foot of coated glass, I let them stand about a minute and a half, while I was adjusting some part of the apparatus, in order to make the discharge; when they exploded by the bursting of one of the number. I observed also, that the jar which was burst was at a considerable distance from the place where I saw the flash at the wires. It was also broke through in two different places.

FOR the same reason, there is no being sure that jars, which have stood one discharge, will bear another equally high. I am confident that several of mine have burst with a much less charge than they had actually held before.

JUNE the 29th. A jar of an ordinary size, which had been in constant use for several months, and which had discharged itself more than a hundred times without any injury, at length burst, as I was discharging it at the prime conductor. The hole was at a different place from that at which the discharge was made, but this does not always happen. The tip of my little finger happened to lie very

lightly on the place, and I felt it was burst by a small pricking, as of a pin, though the explosion at the conductor was nearly equal to that of any other discharge. The coating of a jar contiguous to one that is burst is always melted by the explosion.

JUNE the 25th. A small thin phial, which had been charged singly as high as it could bear, so as to have discharged itself, and also in conjunction with four others of its own size, burst by a spontaneous explosion, when it was charged in conjunction with a battery.

I HAD never heard of a jar bursting in more than one place, or more than one jar in a battery bursting at the same time; but I have often found, to my cost, that this event is very possible. In this case, there must be a discharge at more places than one at the same time: and, besides, it seems to follow from it, that whenever there is a solicitation, as we may say, to discharge at one place, the effort to discharge at every other place is increased at the same time.

IT has frequently happened with me, that jars have been burst at the instant that I was making the discharge in the common way; and when I have come to charge them again, they have appeared to be burst, in some place of the battery where I never expected it. Two instances of this kind happened in the explosion mentioned above; but the most remarkable fact of this kind happened the 31st of May, when a battery of about forty jars,  
each

each containing a square foot of coated glass, discharged itself.

UPON examination, I found that six of the jars were burst, one had the tinfoil coating on the outside quite melted, in a circular spot about half an inch in diameter; and in the inside it was burned quite black, near an inch and an half. A second was melted on the outside, about three quarters of an inch in diameter, and the black spot in the inside was two inches. A third had one hole made in the form of a star, more small cracks like *radii* proceeded from a center than could be counted. And there was hardly one of the jars that was burst with a single hole. Some were burst in seven or eight different places, of which some were very remote from others; but generally there was one principal hole, and several smaller, but independent ones, in the neighbourhood of it, as within half an inch, an inch, or two inches from it.

JUNE the 14th. The above mentioned battery discharged itself once more; when three jars were burst, and one of them, besides its principal hole, had a circular row of fractures, quite round the hole, at the distance of about half an inch. This appearance struck me as something very remarkable, but some light may perhaps be thrown upon it by a subsequent course of experiments. Each of the smaller fractures was about a tenth of an inch in length.

NOVEMBER the 17th. Having charged both my batteries, one of them, at that time, of

thirty-one square feet, and the other of thirty-two, I made the explosion ; when one jar in each battery was afterwards found to be broken. They broke at the instant of the discharge, so that I did not suspect what had happened. Both the batteries had frequently borne a much higher charge. In one of the smaller jars, the coating, besides being burst opposite to the hole, was rent about an inch and an half along a crack that was made from it.

WHEN jars disposed in batteries have been burst in this manner. I have never failed to observe one circumstance which appears to me truly remarkable. It is, that though, in this case, several passages be opened for restoring the equilibrium of the electric fluid, yet the whole seems to pass in the circuit that is formed for it externally. At least, the effect of the explosion is not sensibly diminished, upon any substances that are exposed to it. This I had a fair opportunity of observing when I was transmitting the explosion of the battery through wires of different metals. I found that the utmost force of the battery would do little more than melt a piece of silver wire on which I was trying it, and yet it was, at one time, totally dispersed by an explosion, in making which three jars were broken in different parts of the battery.

THE most remarkable fracture I remember, was of a jar an eighth of an inch thick, and which was therefore, for a long time, thought to be too thick for use. This jar, however, which

which had never held but a moderate charge, burst spontaneously ; when there was found in it one hole like what is commonly observed, from which extended two cracks that met on the opposite side of the jar, so that it came in two parts : but, besides this, there were two other holes, barely visible to the naked eye, at the distance of some inches from the principal hole, and considerably distant from one another. Yet these holes, when examined with a microscope, were plainly fractures, like others made in the same manner, having a white speck in the middle. One of them was above the external coating, but not above the internal.

I HAD frequently been much surprised at the great distance at which several of my jars would discharge themselves, one of five inches being very common. This induced me to try at what distance I could make that spontaneous discharge.

FEBRUARY the 21<sup>st</sup>. I got a jar, eight inches and a half in depth, and three in diameter. Finding it discharged itself very easily, when coated in the usual manner, that is, about four inches from the top ; I cut the coating away, till I had brought it within two inches and a quarter from the bottom ; when it still retained the same property ; and, at length, it burst by a discharge through a white speck of unvitified matter, an inch and three quarters above the top of the coating.

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I THEN procured a jar made blue with zaffre, seven inches and a half high, and two inches and an half in diameter. I coated of it only one inch and a quarter from the bottom, and yet it discharged itself very readily. I afterwards, by degrees, cut the coating down to little more than half an inch from the bottom, and still the discharge continued to be made as before. This property it retained till the month of October following, when it was broke by an accident.

I HAVE another blue jar, of nearly the same size with the former, which is almost full of brass dust, but has no coating at all on the outside. Yet, if I set this jar upon the table, in contact with a single piece of brass chain, going quite round it, and lying upon the table, it will discharge itself the whole length of the glass. N.B. The manner in which the uncoated part of these jars becomes charged exhibits an exceedingly beautiful appearance, especially in the dark; the fire flashing from the top of the coating, in the form of branches of trees, first on one side of the glass, and then on the other, and growing larger and larger till they go over the top.

I HAVE made some experiments, to try how thick a plate of glass may be charged, but I have not been able to ascertain this circumstance with any degree of exactness: I only found, that I was not able to give the least charge to a plate of glass half an inch thick,



thick, when it was not warmed. It was the bottom of a large glass tumbler; but meeting with it only upon a journey at the house of an ingenious electrician, I had no opportunity of making many experiments upon it. I imagine that warming it would have made it capable of being charged. Glass of a quarter of an inch thick will hold a pretty good charge.

MR. KINNERSLEY'S experiments leaving me no reason to doubt, but that Florence flasks were capable of receiving a charge like any other thin glass, which might be made a conductor by heat, I imagined I could soon construct a very strong and very cheap electrical battery out of them. Accordingly I procured a few, for a specimen, but was greatly surprised to find that the electricity went through them, when quite cold, like water through a sieve, without making any fracture in them: for they continued to hold the same small charge, which was different in different flasks. Mentioning this disappointment to Mr. Canton, he informed me, that he had met with the same, and that the permeability of this kind of glass to the electric fluid was owing to small unvitrified parts which may be seen in them. I thought it might be of use to publish this fact, as it may prevent other persons from being disappointed in the same expectations.

As glass had generally been charged when it was smooth, and electrics which had the property of rough glass, when excited, were

exceedingly difficult to charge; I had the curiosity to try what might be done with *rough glass* itself. Accordingly, I first made part of a jar rough, connecting the inside with the outside coating; thinking that the roughness might possibly promote a spontaneous discharge; but I found it was not made in that place preferably to any other. I afterwards took the polish from all that part of the outside of a jar that was above the coating, but it was charged and discharged exactly as before. Lastly, I made a plate of glass rough on both sides, taking off all its polish, and found that it received a charge as well as a smooth plate.

THE manner in which tubes and plates of glass have broken, when I have failed to strike a metallic tinge into them, by the discharge of an electrical battery, have sometimes been attended with circumstances which I cannot easily account for. The following are the facts.

DECEMBER the 3d. Endeavouring to fix a metallic tinge upon a flat piece of glass, it was broken by the explosion, parallel to the line along which the metal lay, at about an inch distance, but not where the tinge itself was.

IN attempting to give a metallic tinge to a part of a long glass tube, it broke, though not in the place where the tinge was made, but on the opposite side, which was shattered all to pieces. The leaf gold had been bound tight to the glass, under a piece of pasteboard,  
which

which covered the gold, but not all the tube. Another tube also was broken in large fragments where the metal had been put on, but into small splinters on the opposite side; and for the space of six or seven inches farther, it was not broken at all on the side of the metal, but very much on the other side.

AT another time, in attempting the same thing with another glass tube; the end of it, which was near a foot distant from the place where the metal was laid, and which was a little cracked in an oblique direction, broke off in a round piece.

As few experiments have been published about *melting wires*, and procuring globules of metal by electrical discharges, and as several things have occurred in my attempts that way, which perhaps have not occurred to other persons, I shall mention a few of the most material circumstances. They will, at least, serve as a direction to those who may be disposed to attempt the same thing.

I HAD frequently attempted to procure those beautiful *globules of metal*, some of which I had seen with Mr Canton; and for that purpose had made the discharge through small wires laid in the bottom of china bowls, &c. but always without success. At length, I thought of inclosing the wires in small tubes; and this expedient fully answered my purpose: for, November the 12th, discharging a battery of thirty-two square

square feet through an iron wire inclosed in a small glass tube, I found innumerable globules of the metal, of very different sizes. The whole piece melted was about two inches. Breaking the glass tube, I found the inside surface uniformly covered with those globules, and a black dust, both fixed into the glass; so that they they could not be separated, without tearing away part of the glass.

THINKING to avoid this inconvenience, I fixed the small wire in the center of a glass tube, of, at least, a quarter of an inch in diameter; but, upon the discharge, this tube, though much wider than the former, was uniformly covered with the globules, and the black dust, which stuck very fast to it, though the metal did not seem to have penetrated into the substance of the glass. When the tube was broken, I scraped off part of the black lining, and the part next the glass looked like a thin plate of the metal.

IMAGINING that the melted metal would not adhere so closely to a conducting substance, as it had to the glass, I inclosed the wire in a paper tube a quarter of an inch wide. Upon opening it after the discharge, it was found uniformly covered with that black dust, and the stain was every where indelible. Sparks of fire had been seen three feet from the place of discharge, but no part of the metal could be found.

I THEN confined the wire closer, wrapping it tight in paper. Upon the discharge, a  
great

great number of sparks were seen, for about a second of time, a quarter of a yard from the paper, which was burned through in several places. Very few pieces could be found, but those were pretty large and irregular. I now found, that, in order to produce these globules, the charge must be moderate, that when the charge was very high, the whole substance of the wire was dispersed in particles too small to be found; and, on the other hand, when the charge was not sufficient, the metal was melted into fragments too large to form themselves into regular globules.

WITH the same battery I once melted a piece of iron wire one seventieth of an inch in diameter, when a piece of it was thrown quite across the table, to the distance of about six feet; where it fell upon a bureau, then tumbled down to the ground, and continued glowing hot all the time. At other times, sparks from melted iron have been thrown three yards, in opposite directions, from the place of the fusion, and continued a sensible space of time red-hot upon the floor.

AT another time I had a very fine opportunity of observing what part of the conductors which form an electric circuit are most affected by the explosion: for, upon discharging a battery of fifty-one square feet through an iron wire nine inches long, the whole of it was glowing hot and continued so for some seconds; the middle part growing  
cool

cool first, while both the extremities were sensibly red. Upon examining it afterwards, both the extremities were found quite melted; an inch or two of the part next to them were exceedingly brittle, and crumbled into small pieces upon being handled; while the middle part remained pretty firm, but had quite lost its polish, so that it looked darker than before.

## SECTION

## SECTION VIII.

## EXPERIMENTS ON ANIMALS.

**A**S I have constructed an electrical battery of considerably greater force than any other that I have yet heard of, and as I have sometimes exposed animals to the shock of it, and have particularly attended to several circumstances, which have been overlooked, or misapprehended by others; it may not be improper to relate a few of the cases, in which the facts were, in any respect, new, or worth notice,

JUNE the 4th. I killed a rat with the discharge of two jars, each containing three square feet of coated glass. The animal died immediately, after being universally convulsed, at the instant of the stroke. After some time, it was carefully dissected; but there was no internal injury perceived, particularly no extravasation, either in the abdomen, thorax, or brain.

JUNE the 19th. I killed a pretty large kitten with the discharge of a battery of thirty-three square feet; but no other effect was observed, except that a red spot was found on the pericranium, where the fire entered. I endeavoured to bring it to life, by distending the lungs, blowing with a quill into the trachea,

trachea, but to no purpose. The heart beat a short time after the stroke, but respiration ceased immediately.

JUNE the 21<sup>st</sup>. I killed a small field-mouse with the discharge of a battery of thirty-six square feet, but no other effect was perceived, except that the hair of the forehead was singed, and in part torn off. There was no extravasation any where, though the animal was so small, and the force with which it was killed so great. This fact, and many others of a similar nature, make me suspect some mistake, in cases where larger animals are said to have had all their blood vessels burst by a much inferior force.

In all the accounts that I have met with of animals killed by the electric shock, the victims were either small quadrupeds, or fowls; and they are all represented as killed so suddenly, that it could not be seen how they were affected previous to their expiration. In some of my experiments, the great force of my battery has afforded me a pretty fair opportunity of observing in what manner the animal system is affected by the electric shock, the animals which I have exposed to it being pretty large; so that a better judgment may be formed of their sensations, and consequently of the immediate cause of their death, by external signs. I do not pretend to draw any conclusion myself from the following facts. I have only noted them as carefully as I could for the use of physicians and anatomists.

JUNE



JUNE the 26th. I discharged a battery of thirty-eight square feet of coated glass, through the head, and out at the tail of a *full grown cat*, three or four years old. At that instant, she was violently convulsed all over. After a short respite, there came on smaller convulsions, in various muscles, particularly on the sides; which terminated in a violent convulsive respiration, attended with a rattling in the throat. This continued five minutes, without any motion that could be called breathing, but was succeeded by an exceedingly quick respiration, which continued near half an hour. Towards the end of this time, she was able to move her head, and fore feet, so as to push herself backwards on the floor; but she was not able to move her hind feet in the least, notwithstanding the shock had not passed through them. While she continued in this condition, I gave her a second stroke, which was attended, as before, with the violent convulsion, the short respite, and the convulsive respiration; in which, after continuing about a minute, she died.

BEING willing to try, for once, the effect of a much greater shock than that which killed the cat upon a large animal, I gave an explosion of sixty-two square feet of coated glass to a dog of the size of a common cur. The moment he was struck, which was on the head (but, not having a very good light, I could not tell exactly where) all his limbs were extended, he fell backwards, and lay with-

without any motion, or sign of life for about a minute. Then followed convulsions, but not very violent, in all his limbs; and after that a convulsive respiration, attended with a small rattling in the throat. In about four minutes from the time that he was struck, he was able to move, though he did not offer to walk till about half an hour after; in all which time, he kept discharging a great quantity of saliva; and there was also a great flux of rheum from his eyes, on which he kept putting his feet; though in other respects he lay perfectly listless. He never opened his eyes all the evening in which he was struck, and the next morning he appeared to be quite blind, though seemingly well in every other respect.

HAVING dispatched the dog, by shooting him through the hinder part of his head, I examined one of his eyes (both of which had an uniform blueish cast, like a film over the pupil) and found all the three humours perfectly transparent, and, as far as could be judged, in their right state; but the *cornea* was throughout white and opaque, like a bit of gristle, and remarkably thick.

BEFORE this experiment, I had imagined, that animals struck blind by lightning had probably a *gutta serena*, on account of the concussion which is seemingly given to the nervous system by the electric shock; whereas this case was evidently an inflammation, occasioned by the explosion being made

made so near the eyes, terminating in a species of the *albugo*; but which I suppose would have been incurable. One of the eyes of this dog was affected a little more than the other; owing, probably, to the stroke being made a little nearer to one eye than the other. I intended to give the stroke about an inch above the eyes.

IN order to ascertain the effects of electricity on an animal body, I, after this, began a course of experiments on the conducting power of its constituent parts; and for some time imagined that a piece of spinal marrow of an ox conducted sensibly worse than the muscular flesh; but after a great number of trials with pieces of spinal marrow from various animals, and pieces of muscular flesh, of the same size and form, and in various states of moisture and dryness, I gave up that opinion as fallacious; but I cannot help wishing the experiments were resumed with some more accurate measure of conducting power than hath yet been contrived.

BEING willing to observe, if possible, the immediate effect of the electric shock on the heart and lungs of animals, I gave, June the 5th, a shock from six square feet to a frog, in which the thorax had been previously laid open, so that the pulsation of the heart might be seen. Upon receiving the stroke, the lungs were instantly inflated; and, together with the other contents of the thorax, thrown

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quite

quite out of the body. The heart, however, continued to beat, though very languidly, and there was no other sign of life for about ten minutes. After that, a motion was first perceived under its jaws; which was propagated, by degrees, to the muscles of the sides; and at last the creature seemed as if it would have come to life, if it had not been so much mangled. The stroke entered the head, and went out at the hind feet.

JUNE the 6th. I discharged a battery of thirty-three square feet through the head and whole extended body of another frog. Immediately upon receiving the stroke, there was, as it were, a momentary distention of all the muscles of the body, and it remained shrivelled up in a most surprising manner. For about five minutes there appeared no sign of life, and the pulsation of the heart could not be felt with the finger. But afterwards, there first appeared a motion under the jaws, then all along the sides, attended with convulsive motions of the other parts, and in about an hour it became, to all appearance, as well as ever.

THE same day, I gave the same stroke to two other frogs. They were affected in the same manner, and perfectly recovered in less than three hours.

THESE facts surprised me very much. I attribute the recovery of the frogs partly to the moisture, which always seems to cover  
their

their body, and which might transmit a good part of the shock; and partly to that provision in their constitution, whereby they can subsist a long time without breathing. To ascertain this, I would have given the shock to toads, serpents, fishes, &c. and various other exanguious animals, but I had not an opportunity. Besides, it is paying dear for philosophical discoveries, to purchase them at the expence of humanity.

## SECTION IX.

EXPERIMENTS ON THE CIRCULAR SPOTS  
MADE ON PIECES OF METAL BY LARGE  
ELECTRICAL EXPLOSIONS.

**I**N the courses of experiments with which I shall present the reader in this and the two following sections, I can pretend to no sort of merit. I was unavoidably led to them in the use of a very great force of electricity. The first new appearance was, in all the cases, perfectly accidental, and engaged me to pursue the train; and the results are so far from favouring any particular theory or hypothesis of my own, that I cannot perfectly reconcile many of the various phenomena to any hypothesis.

FROM the first of my giving any particular attention to electrical experiments, I entertained a confused notion, that a person would stand the best chance for hitting upon some new discovery, by applying a greater force than had hitherto been used. Considering the prodigious number of electrical machines that were in the hands of so many ingenious men, in different parts of the world, I imagined that all that could be done *in little* had been tried; and that the usual experiments had been diversified and combined in almost

almost every method possible; whereas, since electrical machines, I observed, had, of late years, been gradually reduced into less and less compass, a great power of electricity would be almost a new thing, and might therefore supply the means of new experiments. Even Dr. Franklin's force, I considered, was small, in comparison of what might easily be raised, and without a very great expence.

WITH these general and random expectations, I kept gradually increasing my quantity of coated glass, till I got a battery of thirty, forty, sixty, and at length near eighty square feet; and the reader will, in some measure, have seen already, that I was not wholly disappointed in it. The following courses of experiments are more remarkable instances of the advantage I derived from this power of electricity.

THE first remarkable fact that I was by this means led to discover, is that of the circles with which pieces of metal that receive electrical explosions are marked. I shall faithfully relate all the circumstances, and varieties in which it has been exhibited, and the observations I have made upon it; and this I cannot do better than by writing the narrative, in the order in which the appearances occurred.

JUNE the 13th, 1766. After discharging a battery, of about forty square feet, with a smooth brass knob, I accidentally observed upon it *a pretty large circular spot*, the cen-

ter of which seemed to be superficially melted, in a great number of dots, larger near the center, and smaller at a distance from it. Beyond this central spot was a circle of black dust, which was easily wiped off; but, what I was most struck with was, that, after an interruption of melted places, there was an intrie and exact circle of shining dots, consisting of places superficially melted, like those at the center. The appearance of the whole, exclusive of the black dust, is represented, Plate I. fig. 5. No. 1.

JUNE the 14th. I took the spot upon smooth pieces of lead and silver. It was, in both cases, like that on the brass knob, only the central spot on the silver consisted of dots disposed with the utmost exactness, like *radii* from the center of a circle, each of which terminated a little short of the external circle.

EXAMINING the spots with a microscope, both the shining dots that formed the central spot, and those which formed the external circle, appeared evidently to consist of *cavities*, resembling those of the moon, as they appear through a telescope, the edges projecting shadows into them, when they were held in the sun.

THE most beautiful appearance of this kind was exhibited by a spot, which I took on a gold watch case. Besides the cavities, there were, in several places of the spot, hollow *bubbles* of the metal, which must have been raised when it was in a state of fusion.

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These looked very beautiful when examined with a microscope in the sun, and were easily distinguished from the cavities, by having their radiant points (which were very remarkable, and dazzling to the eye) on opposite sides to those of the cavities, with respect to the sun. The whole progress seems to have been first a fusion, then an attraction of the liquid metal, which helped to form the bubbles; and lastly the bursting of the bubbles, which left the cavities. N. B. By this explosion half an inch of a steel wire, one seventieth of an inch in diameter, was melted, and entirely dispersed. In the dispersion, sparks of it were seen red-hot, above half a yard from the place where the wire had lain. This circumstance I have frequently observed since.

I TOOK the circular spot upon polished pieces of several metals, with the charge of the same battery, and observed that the cavities in them were some of them deeper than others, as I thought, in the following order, beginning with the deepest, *tin, lead, brass, gold, steel, iron, copper, silver.*

I WILL not be very positive as to the order of some of the metals, but silver was evidently not affected a fourth part so much as gold, and much less than any of the others. The circles were marked as plain, but the impression was more superficial. Qu. Is this owing to the heat being sooner diffused equably through a piece of silver, than through the substance of any other metal?

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I THOUGHT there might possibly be some difference in the circles on metals which had been a long time in a solid form, and those which had been lately fluid; and to ascertain it, I made the explosion between a piece of lead just solid after melting, and another smooth piece, that I had kept a considerable time.

THE piece of fresh lead was melted more than the other, but there was no other difference between them.

THE semi-metals, as *bismuth* and *zink*, received the same impression as the proper metals; being melted about as much as iron.

I MADE three discharges between a piece of highly polished steel, and a piece of very smooth iron; and in all the cases thought the steel was more deeply melted than the iron. I mention this experiment more particularly, on account of the singular, and beautiful appearance of the circular spot upon the steel in two of the discharges. A circular spot, of about an eighth of an inch in diameter, was uniformly melted, and pretty well defined; and there was a space round this central spot, of the same breadth, uniformly filled with small melted places; but in one of them twice as large as in the other. They exhibited the exact appearance of a planet surrounded by a dense atmosphere; such as, I think, I remember seeing the figures of, in the plates of Burnet's Theory of the Earth. The other circle upon the steel was a common one.

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WHEN the kitten above mentioned was killed, there was no circular spot, or any fusion on the brass knob. I have always found it the most perfect, when the circuit has been composed of the best conductors, and had the fewest intervals.

JUNE the 19th. Hanging the case of one watch upon the brass knobs communicating with the inside of the battery, and receiving the explosion from it upon the case of another watch, which was sometimes of the same, and sometimes of a different metal, and measuring the circles afterwards; I found them to be very nearly of the same diameter. The small varieties seemed to be accidental; or at least did not depend, either upon the metal, or the direction of the electric fluid. But I thought it pretty evident, from a great number of experiments, that the metal which communicated with the outside of the battery, and which I held in my hand to take the explosion, was marked the more distinctly of the two.

It seemed that, when the battery was charged very high, the central spot was the most irregular, many of the dots which composed it spreading into the outer circle, and some dots appearing beyond the outer circle, and very much effacing it; so that the best way to procure a distinct circle, is to take a moderate charge of a very large battery. This may be the reason why the outer circle cannot be perceived, when only small jars are used; the circumference of the circle being  
very

very small, and the charge generally too high. In a very weak charge, it is too faint to be perceived. I have sometimes, however, seen a very distinct circle made by only two jars, each containing half a square foot of coated glass.

THE *diameter* of the spot seems to depend upon the quantity of coated glass; but in what proportion, I have not yet accurately ascertained.

I HAVE observed a good deal of variety in the external circles. Sometimes they have consisted of pretty large dots, disposed at nearly equal distances, in an exact circle; which, in the spaces betwixt each large dot, was completed by smaller dots, visible only by a microscope. But, generally, the external circle consists of a space full of dots, placed irregularly, but so that a line drawn through the midst of them makes a pretty exact circle round the central spot.

PRESENTLY after I had observed the *single circle*, I imagined that, whatever was the cause of the appearance, it was not improbable, but that *two or more concentric circles* might be procured, if a greater quantity of coated glass was used, or perhaps if the explosion was received upon metals that were more easily fused than brass. Accordingly, June the 27th, taking the moderate charge of a battery, consisting of about thirty-eight square feet, upon a piece of tin, I first observed a second outer circle, at the same distance from the first, as the first was from the central

central spot. It consisted of very fine points hardly visible, except when held in an advantageous light; but the appearance of the whole was very beautiful, such as is represented, Plate I. fig. 5. No. 2.

JUNE the 28th. I got another double circle, on a flat pewter standish, much plainer than the former, the outer being about the same distance from the inner, as the inner was from the outside of the central spot.

HAVING hitherto found the circles the most distinct on metals that melt with the least degree of heat, I soon after procured a piece of that composition which melts in boiling water; and having charged sixty square feet of coated glass, I received the explosion with it, and found, what I was endeavouring to get, *three concentric circles*; the outermost of which was not quite so far from the next to it, as that was from the innermost. All the space within the first circle was melted; but the space was very well defined, and by no means like a central spot, which in this case was quite obliterated. The appearance of these three concentric circles is represented, Plate I. fig. 5. No. 3.

I HAVE several times since found parts of three concentric circles upon brass knobs, when I have used no more than thirty square feet of coated glass. They seem to be more easily perceived, when the knobs are a little tarnished: for then the small dots, in which the metal is melted, are more easily distinguished,

guished, especially when they are held in a proper light with respect to the sun.

I MADE many attempts to make these circles larger than I had usually found them upon pieces of metal, chiefly by means of worse conductors; thinking that the electric matter not being so well conducted, and passing with less rapidity, would spread wider. This was probably the case, but then it is likewise probable, that I wanted force to make such an impression visible. For this purpose, however, I received the explosion between two pieces of raw flesh, two potatoes, two moist bladders, and things of a similar nature, but without any effect whatever; no mark at all, or, at least, nothing regular remaining upon them. When I took the explosion upon a piece of wood charcoal, it seemed to be melted, and run in small heaps, within the space of about the usual diameter of a circular spot; and when I took it upon a piece of pit charcoal, a piece seemed to be struck out of it, and a hole was left in it; but there was no regular circle upon either of them; nor was there any sensible ignition in either case.

At one time I laid a piece of *lead ore*, scraped very smooth, upon the wires of the battery, and took the explosion with a piece of tin ore scraped in the same manner; but though I examined the places with a microscope, I could not be sure that there was any part melted, much less any regular circular spot; but there lay on both of them a yellow matter, like sulphur, round the place where  
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the explosion was taken, and a very disagreeable smell was excited. This probably arose from a mixture of the sulphur of the lead ore, and the arsenic of the tin ore.

I RECEIVED the explosion *in vacuo*, at the distance of about three inches; but found no regular circular spot, owing, probably, to the two interruptions I was obliged, in this case, to make in the circuit, one in the air, and the other within the receiver; by means of which the effect of both would be weakened, the whole force being, as it were, divided between them: for in all such cases, though both the explosions were made in the open air, I found the circles less perfect.

AFTERWARDS, I contrived to make the explosion in one additional atmosphere of condensed air, but the circles were smaller, and less distinct than the other two circles, which I was obliged, at the same time, to make at the other interruption of the circuit, in the open air. The denser air would probably confine the electric matter within a narrower compass; in the same manner as the common air prevents that diffusion of it which is remarkable *in vacuo*.

THE distance at which the discharge was made occasioned no difference in the diameter of these circular spots. When, by putting a drop of water upon the brass rod communicating with the inside of the battery, I made the discharge at the distance of two inches, the spot was just the same as if it had been received

ed at the distance of half an inch, i. e. about a quarter of an inch in diameter.

I ALWAYS found that if the explosion was obliged to pass through any bad conductor before it reached the metal, the impression it made upon it was contracted, and deeper than if it had been received immediately by the metal. This was evident when paper, a piece of bladder, or varnish were put upon the brass rods with which the discharges were made; though a very thin coating of varnish or moisture did not intirely prevent the appearance of the circles.

IN making a course of experiments with bad conductors, and in using various methods to promote the discharge of the battery at greater distances than usual, I was peculiarly struck with some phenomena which occurred in the use of water.

I PUT a drop of water, about a quarter of an inch in diameter, upon the brass rod communicating with the inside of the battery, and took the explosion directly over it. The discharge was made at the distance of about an inch, and the extremity of the drop was marked with a most beautiful circle, exceedingly well defined on the inside, and vanishing gradually outwards, like a fine shade in drawing. But what struck me most in the appearance was, that, in this circle, there was no central spot.

NOT knowing what this new circumstance was owing to, I wetted a piece of smooth copper,



copper, which lay upon the wires of the battery, and taking the explosion upon it, I only found a long streak at the edge of the wetted place, well defined on the side of the water, but vanishing gradually on the opposite side, as in the former case. In this, and other similar experiments, I observed that the electric matter avoided the water, and would go a greater way in the air, in order to come at the metal.

I THEN laid more water upon the copper, but so as only to moisten it; for the surface, being convex, would not allow it to lie in any great quantity; and upon taking the explosion, I found no circle, but several beautiful circular spots melted very deep, one of which was much larger than the rest. These experiments seem to show, that the electric matter meets with a considerable resistance in passing through water, which confines its excursion more than the air; and that, by such a condensation, its force is greatly increased, so as to leave deeper impressions upon the metal than when it had passed only through the air; in like manner, if two pieces of metal be placed, nearly in contact, or if they be light, and one of them lie upon the other, the impression made upon both of them, by the discharge of the battery passing through them, will be considerably deeper than it would have been if the electric matter had not been confined to so small a compass as the points in contact.

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To account for the formation of these concentric circles, nothing seems to be necessary, but the supposition of the elasticity of the electric fluid, whereby its particles repel one another. For then, supposing a quantity of electric matter to issue from one piece of metal to another, through the air, it will endeavour to spread, but will be confined in its passage by the surrounding electric medium, and the strong attraction of the opposite metal. If this piece of metal have a flat surface, or one that is nearly so, the fluid will be attracted by it pretty equally, within a certain space; so that the mutual repulsion of its particles will have room to exert itself, and produce a division of the whole quantity; and as this repulsion is the same in all directions, the effect must be its throwing itself into a circle, or several concentric circles, on its entering the opposite piece of metal, and consequently melting it, in that form. For the same reason the circles themselves will consist of separate dots, each of which might have been caused by the fluid in another hollow circle, but being so small, the fusion of the metal could not show that circumstance.

Of the circles being formed in this manner, I have been in a manner an eye witness, when I have presented a flat piece of metal to a large prime conductor strongly electrified, and have seen the large sparks, five or six inches long, divide about the middle, and strike the metal in a circle, about an inch in diameter;

ter; generally with a central spot, but sometimes without one.

THE manner in which several of the jars, mentioned in a former section, were broken seems to be analogous to the formation of these circles. I mean those that were pierced with a number of small holes in the neighbourhood of the principal one; but more especially that which was broken with an intire circle of small and independent fractures round the principal hole.

THE remarkable story of the five peasants of whom the first, third, and fifth were killed by lightning, as they were walking in a right line; and which was mentioned before, as analogous to a fact observed by Mr. Monnier, will perhaps be thought more analogous to this. For supposing the diameter of the concentric circles formed by lightning to be sufficiently great, and the central spot to fall upon the third person, the two on each side of him would escape, by being in the first interval round the central spot; while the two who walked first and last would fall into the circumference of the first circle. But other facts lead me to think, that all these effects may have been produced by a constant stream of electric matter, in a progressive motion, the first part of which being discharged upon any object, the stream is weakened when it meets with the second, but is accumulated again when the third comes in its way, and so alternately, as

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long

long as the stream and the line of objects continues. This account seems to be rendered more probable by the number of objects that have been struck in this manner, particularly in a case which is related by Dr. Wallace in his account of the Orkney islands, p. 78. "In the year 1680," he says, "the lightning entered a gentleman's cow-stall where were twelve cows standing side by side, as they used to be, and killed every other one; that is, it killed the first, and missed the second; it killed the third and missed the fourth; and so of the rest, so that six were killed, and six remained alive and untouched."

COMMUNICATING this experiment to Dr. Price, he suggested to me, that the circles called *fairy rings*, which consist of grass of deeper green in pasture fields, and which have by some been imagined to be occasioned by lightning, might be analogous to the circles above mentioned, but that they want a central spot. I have since examined one of these rings. It was about a yard in diameter, the ring itself about a quarter of a yard broad, and equally so in the whole circumference; but there was no appearance of any thing to correspond to the central spot.

I HAVE since met with a curious article in the Philosophical Transactions, relating to those fairy circles, communicated by Mr.

Jeffop, which confirms the supposition of their being occasioned by lightning, and with which I shall therefore conclude this section.

“ I HAVE often been puzzled to give an account of those phenomena, which are commonly called fairy-circles. I have seen many of them, and those of two sorts; one sort bare, of seven or eight yards diameter, making a round path something more than a foot broad, with green grass in the middle; the others like them, but of several bignesses, and encompassed with a circumference of grass, about the same breadth, much fresher and greener than that in the middle. But my worthy friend Mr. Walker, gave me full satisfaction from his own experience; it was his chance one day, to walk out among some mowing grass (in which he had been but a little while before) after a great storm of thunder and lightning; which seemed by the noise and flashes to have been very near him: he presently observed a round circle, of about four or five yards diameter, the rim whereof was about a foot broad, newly burnt bare, as the colour and brittleness of the grass roots did plainly testify. He knew not what to ascribe it unto but to the lightning, which, besides the odd capricious remarkable in that fire in particular, might without any wonder, like all other fires,

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“ move

“ move round, and burn more in the ex-  
“ tremities than the middle. After the  
“ grafs was mowed, the next year it  
“ came up more fresh and green in the  
“ place burnt, than in the middle, and at  
“ mowing-time was much taller and ran-  
“ ker \*.”

• Phil. Transf. abridged, Vol. ii. p. 182.

SECTION

## S E C T I O N X.

EXPERIMENTS ON THE EFFECTS OF THE  
ELECTRICAL EXPLOSION DISCHARGED  
THROUGH A BRASS CHAIN, AND OTHER  
METALLIC SUBSTANCES.

FROM the very first use of my battery, I had observed a very *black smoke* or *dust* to arise upon every discharge, even when no wire was melted, and the brass chain I made use of was of a considerable thickness. Of this circumstance, however, I only made a slight memorandum, as what I could not then account for, and paid no particular attention to it; till on the 13th of June 1766, I was struck with another casual appearance, as I was intent upon the experiments relating to the circles above mentioned.

I OBSERVED, that a piece of white paper, on which lay the chain I was using to make the discharge, was marked with a *black stain*, as if it had been burnt, wherever the links had touched it. Yet I could not then think that it could be burnt by so thick a chain. I imagined the chain must have been dirty, and the dirt have been shaken off by the stroke. Still however I neglected the experiment till, observing a very striking appearance of the same kind, on the 1st of September follow-

ing, I was determined to attend to the circumstances of it a little more particularly than I had done.

I MADE my chain very clean, and wrapping it in white paper, I made a discharge of about forty square feet through it, and found the stain wherever it had touched the paper.

SOME time after, I wrapped the paper, in the same manner, round a piece of brass wire; but, making a discharge through it, saw no stain. To ascertain whether this appearance depended upon the discontinuity of the metallic circuit, on the 13th of the same month, I stretched the chain with a considerable weight and found the paper, on which it lay as the shock passed through it, hardly marked at all.

FINDING that it depended upon the discontinuity, I laid the chain upon white paper, making each extremity fast with pins stuck through the links; and when I had made the discharge, observed that the black stains were opposite to the *body of the wire* that formed the chain, and not to the *intervals*, as I had sometimes suspected.

SEPTEMBER the 18th. Observing that a pretty considerable quantity of black matter was left upon the paper, on every discharge with the same chain; I imagined it must have lost weight by the operation; and to ascertain this circumstance, I took another chain not so thick as that I had used before. It was five feet four inches long, and weighed exactly



ly one ounce, seventeen penny-weights, four grains. After the discharge, I found it had lost exactly half a grain of its weight. The shock had only passed through a part of it, the rest lying on a heap. I then discharged the same shock through its whole length, and weighed it, found it had lost just another half grain. By repeated experiments I found, afterwards, that the surest way to strike off part of its weight, was to make the shock pass through a small part of its length, and that when a considerable length was used the event was uncertain.

N. B. THESE and all the following experiments, except where the contrary is expressed, were made with a battery of *thirty-two square feet*, that force appearing to be sufficient, and the charging of it not taking up much time. At the time of both the above mentioned discharges, an iron wire of one seventieth of an inch in diameter was made red-hot, but was not melted.

OBSERVING how deep a stain was made by the links of a thick brass chain, I had the curiosity to try what would be the consequence of sending a shock through a piece of charcoal. Accordingly I took a small piece, about half an inch in length, and found that, in the discharge, it was all blown to dust. The pasteboard on which it lay was torn, the charcoal being forced into it, so that the impression appeared on the other side. The blackness was spread to a great distance, and the tinge every where indelible.

SEPTEMBER the 21<sup>st</sup>. In making the mark above mentioned, on part of the sheet of paper, on which I had written an account of the experiment to Dr. Franklin, I happened to lay the chain so as to make it return at a sharp angle, in order to impress the form of a letter on the paper; and observed that, upon the discharge, the part of the chain that had been doubled was displaced, and pulled about two inches towards the rest of the chain. At this I was surpris'd, as I thought it lay so, as that it could not slide by its own weight. Upon this I repeated the experiment with more accuracy. I stretch'd the whole chain along a table, laying it double all the way, and making it return by a very sharp angle. The consequence always was, that the chain was shortened about two inches, and sometimes more; as if a sudden pull had been given to it by both the ends.

CONSIDERING that this pull must have been given to it by the several links suddenly repelling one another, at the instant of the explosion, I compar'd the links with the black marks that were made by them upon the table, and found that each link had been pulled from the place on which it had lain, and most of all, at the greatest distance from the place of the explosion.

CONVINCED that the chain had been shortened by the mutual repulsion of the links, I endeavour'd to measure with exactness how much the shortening was, in a given length of chain. To do this, I measured two feet  
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four inches of the chain, as it lay upon the table, in one straight line, without any return, one end being fixed and the other moveable; and found that, upon discharging sixty-four square feet through it, it was shortened a quarter of an inch in its whole length. I had contrived that the suddenness of the motion should not throw one part of the chain upon the other.

SUSPECTING that the black smoke, which rose at every discharge, might come, not from the chain, but from the paper, or the table on which it lay, and which was probably burnt by the contact of it, I let the chain hang freely in the air; but, upon making the discharge, I observed the same black gross smoke that had before risen from the paper or the table. It was therefore part of the metal itself, which had been converted into that black dust.

To give my reader a better idea of the mark made upon white paper by a chain, through which the electric shock is transmitted, I laid a chain upon the original drawing of Plate I. for the engraver to copy as exactly as he could; and he has succeeded pretty well. The breadth of the spots are about the mean thickness of the wire of the chain, and [a, b] marks the place to which that part of the chain which was returned was thrown back, by the sudden repulsion of the links.

I HAD before observed the electric sparks betwixt each link to be most intensely bright,  
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so as, sometimes, to make the whole chain appear like one flame in the dark; but the appearance of the chain at the instant of the shock, as it hung freely in the air, was exceedingly beautiful; the sparks being the largest and brightest at the bottom, and smaller, by degrees, towards the top, where they were scarcely visible; the weight of the lower links having brought them so much nearer together.

SEPTEMBER the 26th. Being still in some doubt whether the blackness that was left on the paper came from the burning of the paper, or something that was thrown from the chain; I once more hung the chain freely in air, and put under it, but so as not to touch it, a piece of white paper, on which I also laid a few pieces of down, to observe whether they would be affected by any electrical attraction or repulsion. On making the discharge, the down was all dispersed, and the paper was marked with a black stain, near the length of an inch; which was the distance at which the two parts of the chain hung from one another, a little above the paper. Some parts of the stain were deeper than others, the whole mark consisting of four different spots of a deeper black, joined by fainter streaks, answering to four links of the chain, which hung nearly parallel to the paper. The stain could not be wiped off with a handkerchief, though it was not so deep as when the chain had touched the paper.

paper. Thus I was satisfied, that a considerable part, at least, of the blackness had come from the chain.

SEPTEMBER the 27th. Willing to ascertain more exactly what part of the chain, the solid links, or the intervals, was most affected by the shock; I dipped it in water, and laying it quite wet upon a piece of white paper, discharged a shock through it. Part of the water was thrown into my face, being scattered in all directions, and all the chain left instantly and perfectly dry. The paper was very much stained for the space of an inch broad, wherever the chain lay; not equably, but as if it had been handled with dirty fingers. The stain was indelible, and where the chain was returned, a hole was struck quite through the paper.

To determine whether the paper, in the above mentioned instances, had really been *burnt*, as well as *stained*. I laid a part of the chain, at the time of the last discharge, upon three half crowns; and found they were all melted, in the places where the chain had touched them. The marks made by the fusion were about the breadth of the chain, and so deep that nothing but a tool could efface them.

To determine, if possible, more sensibly what it was that made the black tinge, I laid the chain upon my hand, when I had a moderate charge; and it was marked just like the paper. I felt a kind of pricking or burning at the instant of the explosion, and the  
painful

painful sensation continued a small space of time.

I MADE no doubt but that with a heat that melted metals, I could easily contrive to fire gunpowder; but, though I laid the chain upon the grains, and rammed the powder about the chain put through a quill, I could not succeed. In the first case, the powder was dispersed; and in the second, the quill was burst, and there was a smell, as after an explosion of gunpowder, but no actual firing of it.

HITHERTO I had always put the chain in contact with bodies that were conductors. I was now willing to try what would be the consequence of laying it in contact with electrics. Accordingly, I dipped the chain in melted rosin, till it had got a coating of a considerable thickness. When it was quite stiff, I laid it carefully, without bending, upon white paper, and made the discharge through it. The rosin was instantly dispersed from all the outside of the chain, it being left as clean as if none had ever been put on. That with which the holes in the chain had been filled, having been impelled in almost all directions, was beaten to powder; which, however, hung together, but was perfectly opaque; whereas it had been quite transparent, before this stroke. I felt some of the rosin fly in my face. The stain upon the paper was very deep, containing a good deal of rosin, and several holes were struck through the paper on which it was laid. A half crown, on which  
part.

part of the chain had lain, was melted, and so deeply stained with the rosin, that it could not easily be cleaned.

I NEXT laid the chain upon a *piece of glass*; and considering how both the half crown and the rosin had been affected, expected it would have been broken to pieces; but instead of that, the glass was marked in the most beautiful manner, wherever the chain had touched it; every spot the width and colour of the link. The metal might be scraped off the glass at the outside of the marks; but in the middle part it was forced within the pores of the glass; at least nothing I could do would force it off. On the outside of this metallic tinge was the black dust, which was easily wiped off.

I HAVE since given the same tinge to glass with a silver chain, and small pieces of other metals; but could not do it with large pieces. They were melted where they touched one another, but the glass was not tinged.

OCTOBER the 7th. I had the curiosity to try, whether I could not give a tinge to glass with quicksilver. In order to this, I laid some globules in a right line, and laid a thin piece of glass upon them, to flatten them, and bring them nearer into contact with one another. Both the slips of glass were shattered in a thousand pieces, and dispersed all over the room, several of them flying in my face; though no part of the quicksilver could be found,

found, except what adhered to some fragments of the glass, to which it had given a kind of uniform whiteness; but no distinct globules could be seen, and it was easily wiped off, so that no part of it was fixed in the glass. My head ached all the remainder of the day, which I attributed to the fumes of the mercury.

SEPTEMBER the 28th. Having dipped the chain in water, and found it instantly dispersed, I wished to see what would be the effect of discharging a shock through a chain quite covered with water. Accordingly, little imagining the consequence, I laid the chain upon a piece of white paper, in the bottom of a china dish, and poured in water just sufficient to cover it. Also, under one part of the chain, and in the water, I put a half crown. Upon the explosion, the water was blown about the room, to a great distance, the half crown was melted in two places, the dish broken into many pieces, and the part that lay immediately under the chain into very small fragments. The paper was a little stained, and the water, I could perceive, had been a little fouled by the black dust.

BEING certain that the dish must have been broken by the concussion given to the water by the electric spark under it, in the manner in which Signior Beccaria's tubes were broken (though I had not seen his work at that time, but had seen the experiment at  
Mr.



Mr. Lane's \*) I had the curiosity to try what would be the effect of making a discharge through the chain hanging freely in water. I therefore got a tin vessel, holding a quart, and letting the chain hang three inches and a half below the surface of the water, made the discharge. The electric sparks appeared intensely bright in the water, all along the chain; some of the water was thrown out, and the vessel appeared to have been pressed with some force upon a book, which I had put under it, a visible impression being made upon it. The vessel must have received a great concussion: for the dust had been shaken from the bottom upon the book, though I had carried the vessel up and down the room, without perceiving that any dust adhered to it.

I WAS willing to repeat this experiment with some variation of circumstances, and fastened a piece of small silver wire to two pieces of strong brass wire, and plunged the whole an inch or two under the surface of the water. Upon the discharge, the silver wire was melted, at least snapped asunder, the vessel had been pressed downwards more violently than before, a considerable quantity of the water was thrown about the table, and some was dashed perpendicularly upwards, against the top of the room; where there were

\* See his account of it, *Phil. Trans.* Vol. lvii. p. 458.

five wet places, each about the bigness of a half crown. I have since frequently melted wires under water, and have even made large pieces of iron wire red-hot in the water.

SEPTEMBER the 29th. I made a discharge through three pieces of the same chain, each being a different circuit. They all left their impression upon the paper, and nearly equal. Also three out of four pieces made pretty equal marks, but the fourth failed intirely.

AT another time, a chain, which communicated with the outside of the battery, but which made no part of the circuit, made the black stain on a piece of white paper on which it accidentally lay, almost as deep as the chain that formed the circuit. I was then melting a piece of wire, which had the same effect as using a bad conductor. The same thing has frequently happened since.

NOVEMBER the 12th. I put a chain through a glass tube, so wide as that it could only touch one side; and upon the discharge, observed four sets of marks, made by the metal being driven into the glass; as if four chains had been in the tube, and all had received the shock. Two of the rows, on one of which I imagine the chain had lain, were better marked than the other two, but all were very plain.

THE last thing that engaged my attention with respect to this course of experiments, was

was that *black dust* which I have observed to be discharged from the brass chain, and other pieces of metal. As it was so extremely light as to rise like a cloud in the air, so as sometimes to be visible near the top of the room; I concluded that it could not be the metal itself, but probably the *calx*, or the *calx* and *phlogiston*, in another kind of union than that which constitutes a metal; and that the electric explosion reduced metals to their constituent principles as effectually as any operation by fire could do it, and in much less time. I was confirmed in this opinion by finding, in the first place, that this black dust collected from a brass chain would not conduct electricity, which is known to be a property of the calces of metals, and also by the result of some of the following experiments.

CONSIDERING this black dust as a proof of calcination, and observing it to be produced when I made the explosions for the circular spots between gold and silver watch-cases, as was related above; I began to think I had made a calcination of those metals, which all the chymists say is impossible: but the following experiments convinced me, that it could only be the alloy that was in them which had yielded the black dust or calx.

SENSIBLE that my experiments with these metals would conclude nothing, unless I got the specimens quite pure, I first procured a

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small quantity of *grain gold*, which I was informed was the purest that the goldsmiths know, and discharged an explosion of the battery through a train of the pieces, an inch and a half in length, laid on a piece of white paper. Only two of the larger grains could be found after the explosion. Two leaves of paper were burnt, or torn through in several places, and more would probably have been torn in the same manner, if I had used more. But what I principally attended to was the tinge that was given to the paper, with a view to which I had made the experiment. The paper was stained near an inch on each side of the train, with black intermixed with red, making an odd motley appearance.

WITH the same view, I laid a similar train of bits cut with a knife from a piece of as pure silver as I could procure. They were dispersed, and the paper burnt through, in the same manner as with the gold; and the space of about an inch on each side of the train was stained with black intermixed with a deep yellow, which was considerably different from the tinge made by the fusion of the gold.

THE blackness in these tinges convinced me, that there had been a calcination of some part of the metal; but I was convinced it must have been some alloy, by an experiment I presently after made with a piece of leaf gold; which, I believe, is generally the purest that

that can be got. A small slip of this I put through a quill, letting a part hang out at each end; and when I had made the discharge through it, I found the quill tinged with a beautiful *vermillion* red, without the least intermixture of black. When I dispersed a slip of leaf brass in the same manner, the greater part of the tinge was black, with a little brown mixed with it in a few places.

IN order to ascertain whether the black dust was a pure calx, or contained a portion of the metal, I procured a small quantity of it, by sending an explosion through some pieces of iron wire, sometimes put into a quill, sometimes laid upon white paper, and sometimes upon glass, or inclosed in glass tubes; but could never be quite sure that there was any part of it that was not affected with the magnet, which the mere calx would not have been.

SOME of the experiments with the brass chain, related in this section, are similar to one of Mr. Wilson's, mentioned vol. I. p. 121, concerning bodies placed without the electric circuit being affected with the explosion. As to the cause of this, and the other appearances above mentioned, I have no conjecture worth communicating to the public. I have only pursued the analogy of facts, and that not very far. Others may compare them, pursue them farther, and ascertain their causes.

MR. CANTON has since clearly proved the calcination of pure gold and silver by the heat of electrical explosions, producing numberless most beautiful globules of transparent glass, and also others tinged with all the varieties of colour from those metals. He has also made it probable, that the *black dust* mentioned in this section, is the calx, or glass of the metal, reduced to smaller particles than the laws of optics require to produce colour.

## SECTION

## S E C T I O N    X L

EXPERIMENTS ON THE PASSAGE OF THE  
ELECTRICAL EXPLOSION OVER  
THE SURFACE OF SOME CONDUCT-  
ING SUBSTANCES, WITHOUT ENTERING  
THEM.

**I** OBSERVED, in relating the experiments on ice, that, in my attempts to ascertain its conducting power, I sometimes saw the flash of the electrical explosion strike directly to the chain, along the surface of the ice. But as this passage on the surface was produced only by a common jar, it was not much greater than the distance at which the discharge was usually made, and the appearance did not strike me. But afterwards the same phenomenon occurred in the use of my battery, where the passage over the surface so far exceeded the usual distance of a common discharge, that it engaged my attention in a very particular manner, and produced some pleasing experiments; which I shall recite in the manner, and nearly in the order in which they happened.

DECEMBER the 11th. Thinking to make a circular spot on a piece of raw flesh, I took a leg of mutton, and laying the chain that communicated with the outside of the battery

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over the shank of it took the explosion on the outward membrane, about seven inches from the chain; but was greatly surprised to observe the electric fire not to enter the flesh, but to pass, in a body, along the surface of it, to come to the chain.

THINKING that this effect might be occasioned by the fatty membrane on which the explosion was taken, I again laid the chain, in the same manner, over the shank, and took the explosion upon the fibres of the muscles, where they had been cut from the rest of the body; but still the fire avoided entering the flesh, made a circuit of near an inch round the edge of the joint, and passed along the surface, to come to the chain as before, though the distance was near a eleven inches.

IMAGINING this effect was promoted by the chain lying lightly on the surface of the flesh, and therefore not really in contact with it; I took another explosion, when the hook of the chain was thrust into the flesh; on which the fire entered the mutton, and, as I held it in my hands, both my arms were violently shocked up to my shoulders; whereas, in the cases of the electric fire passing over the surface of the flesh, my fingers, happening to touch the chain, were only affected with a slight pricking, or superficial burning, which has been explained before.

THIS phenomenon being so remarkable, and the battery by this means discharging  
at



at a distance about twenty times greater than it could usually be made to do, I thought to try other substances, of a conducting power similar to that of raw flesh; and of these, *water* was the most obvious. Accordingly, the next day, I laid a brass rod communicating with the outside of the battery very near the surface of a quantity of water (to resemble the chain lying upon the surface of the flesh, without being in contact with it) and, by means of another rod furnished with knobs, made a discharge on the surface of the water, at the distance of several inches from any part of the rod; when the electric fire struck down to the water, and, without entering it, passed visibly over its surface, till it arrived at that part of the rod which was nearest to the water, and the explosion was exceedingly loud. If the distance at which I made the discharge exceeded seven or eight inches, the electric fire entered the water, making a beautiful star upon its surface, and yielding a very dull sound.

THE resemblance between this passage of the electric matter over the surface of the water, and that which Dr. Stukeley supposed to sweep the surface of the earth, when a considerable quantity of it is discharged to the clouds during an earthquake, immediately suggested to me, that the water over which it passed, and which was visibly thrown into a tremulous motion, must receive a concussion, resembling that which is given to the waters of the sea on such an occasion.

To try this, myself, and other persons who were present, put our hands into the water, at the time that the electrical flash above mentioned passed over its surface; and we felt a sudden concussion given to them, exactly like that which is supposed to affect ships at sea during an earthquake. This percussion was felt in various parts of the water, but was strongest near the place where the explosion was made.

AFTERWARDS, I made the explosion of a jar, containing three square feet of coated glass, at some distance below the surface of the water, so as to be visible in the water, and we felt the same concussion that we had done before, when the fire of the battery passed over the surface, only much weaker. The flash of electric fire in the water does certainly displace some of it, and thereby give a sudden concussion to the rest; and the similarity of the effect is a considerable evidence of a similarity in the cause.

I AFTERWARDS made the fire of a jar pass through the water, making a space of about a foot part of the circuit; when, putting our hands in its passage, they were affected, but in a very different manner from what they were before: for this evidently affected the nerves and muscles of the hand internally, and occasioned a small degree of the same kind of convulsion which is felt by the electric shock itself; whereas the other was a mere percussion, affecting the surface of the hand. Both sensations were, indeed, felt most sensibly

bly at the surface of the water, though our hands were, in some measure, affected by both as low as we could put them.

BEING willing to experience what kind of a sensation this passage over the surface would occasion, I laid a chain in contact with the outside of a jar lightly on my finger, and sometimes kept it at a small distance, by means of a thin piece of glass; and, if I made the discharge at the distance of about three inches, the electric fire was visible on the surface of the finger, giving it a sudden concussion, which seemed to make it vibrate to the very bone; and when it happened to pass on that side of the finger which was opposite to the eye, the whole seemed perfectly transparent in the dark. If I took the distance much larger, the fire entered the finger, occasioning a very different sensation from the former. The one was like a blow, but of a very peculiar kind, whereas the other is well known to be a convulsion.

I THEN ventured to put my fingers upon a piece of the spinal marrow of an ox, while the explosion of the battery was passing over it, when I felt only a slight pricking, or percussion on each side of my finger; and the sensation continued for some time. This sensation did not extend at all beyond the place of percussion; but afterwards, putting two of my fingers on the same piece of spinal marrow, when the charge of the battery was considerably stronger, I received a concussion which

which affected my whole hand, but it was with a kind of a vibratory motion.

PLEASED with this resemblance of the earthquake, I endeavoured to imitate that great natural phenomenon in other respects; and it being frosty weather, I took a plate of ice, and placed two sticks, about three inches high, on their ends, so that they would just stand with ease; and upon another part of the ice I placed a bottle, from the cork of which was suspended a brass ball by a fine thread. Then, making the electric flash pass over the surface of the ice, which it did with a very loud report, the nearer pillar fell down, while the more remote stood; and the ball, which had hung nearly still, immediately began to make vibrations about an inch in length, and nearly in a right line from the place of the flash.

I AFTERWARDS diversified this apparatus, erecting more pillars, and suspending more pendulums, &c. sometimes upon bladders stretched on the mouth of open vessels; and at other times, on wet boards swimming in a vessel of water. This last method seemed to answer the best of any; for the board representing the earth, and the water the sea, the phenomena of them both during an earthquake may be imitated at the same time; pillars, &c. being erected upon the board, and the electric flash being made to pass either over the board, over the water, or over them both. This makes a very fine experiment.

WHEN

WHEN I first made this experiment of the electric flash passing over the surface of water, I thought it necessary, that neither the piece of metal communicating with the outside, nor that communicating with the inside of the jars should touch the water immediately before the discharge. But I afterwards found, that the experiment would answer, though either, or even both of them were dipped in the water: for in this case the explosion would still prefer the surface to the water itself, if the distance was not very great; and would even pass at a greater distance along the surface, when there was a nearer passage from one rod to the other in the water.

JUST before the discharge, both the rods were observed to attract the water very strongly. It was thrown upon the rod communicating with the outside when it was laid near half an inch above the surface. When I put a drop of water on the rod communicating with the inside, the discharge was made at the distance of about two inches from the surface of the water, the fire first descending perpendicularly, and then passing along the surface; and if the rod communicating with the outside had a drop of water upon it, it might be placed higher over the water than if it had not. At the time of the explosion, this drop was elongated, and promoted the discharge very considerably.

My attention was next drawn to the kind of impression which was made upon the water by the passage of the electric fluid in this manner.

manner. To ascertain this, I first placed a shilling level with the water, to receive the explosion before it passed along the surface; and observed that it was melted, but only about half as much as I imagined it would have been in the common way. There was no regular circular spot. And I could never perceive that the brass rod which communicated with the outside of the battery was at all melted by the explosion.

JUDGING from the concussion given to the whole body of the water over which the shock passed, I thought that the trace of it might possibly be preserved on the surface of soft paste; and accordingly I made the explosion pass over the surface of some, and plainly observed, that the part under the passage was depressed; the electric matter having repelled it. The impression was not deeper where the explosion first fell than in any other part of the track.

To distinguish more accurately between the effect of the electric matter when it probably enters the water, and when it only passes over the surface, I spread a little water, exceedingly thin, upon the surface of a smooth piece of slate; but, though the explosion passed over the surface, with its usual violence, I could not perceive that it had occasioned the least degree of evaporation; which Signior Beccaria found to be the consequence of making the electrical explosion through water in such circumstances.

WHEN

WHEN the explosion passed over the surface of the plate of ice, in the experiment of the earthquake above mentioned, the ice seemed to be melted, both where the chain had been laid, and also along the track over which the explosion had passed. But this melting, if it was such, was not uniform; but looked as if a chain with small links had been laid hot upon it; and the impression was not at all deeper where the explosion was first received.

WHEN the explosion passed over the surface of a green leaf, the leaf was rent in two directions; the longer in the track of the explosion, and the other at right angles to it.

I SEVERAL times made the explosion on the surface of snow, when it always dispersed a considerable quantity of it, making a hole near two inches deep, and almost as broad as long; for it could not be made to pass at a greater distance than about three inches.

I WAS not a little surprised to find that I could not make this electrical explosion pass equally over the surface of substances which were conductors in nearly the same degree; and for a long time imagined, that this property was peculiar to water, or to bodies that conducted by means of the water they contained. I could never make it pass the surface of any kind of charcoal; though all the degrees of conducting power may be found in different pieces of it: and I was the more con-

confirmed in my opinion, by observing, that, though the explosion passed perfectly well over the surface of a smooth board, that had been just wetted, and immediately wiped as clean as possible; yet two hours after, when the board was quite dry, it would not pass at all in the same place. It also passed with great violence over the surface of a bladder which had been moistened about a quarter of an hour before, and then seemed to be quite dry; but would not pass in the least degree two or three hours after. In the former case, the explosion had left a mark where it had passed over, darker than the rest of the surface, a kind of polish which was on it being taken off: in the latter case, as the dry bladder conducted very imperfectly, the fire of the charge spread in a beautiful manner, covering a space of about an inch in diameter.

THIS electrical explosion would not pass in the least degree, over the surface of new glass, notwithstanding its property of diffusion above mentioned seemed to promise that it might. Neither would it pass at all over the surface of alum, rock salt, sal ammoniac, blue or green vitriol, or a piece of polished agate; though these are all conductors of a middle kind, like water; and several of them had very smooth surfaces. It also refused the surface of dry wood, and dry leather, even the smoothest cover of a book.

BUT I found that I had concluded too soon, that this passage of the electrical explosion  
was



was peculiar to the surface of water, by finding, first, that it passed over the surface of a touch-stone, and then over a piece of the best kind of iron ore, exceedingly smooth on some of its sides. This piece is about an inch thick, and about three inches in its other dimensions. The full charge of a jar of three square feet would not enter it. It was diverting to observe how the electrical explosion would make a circuit, round its angles, when it was made in a place remote from the jar. It looked like a thing invulnerable.

THIS electrical explosion passed over the surface of oil of vitriol with a dull sound, and a red colour, which was the only appearance of the kind that I have yet met with. In all other cases, if it passed at all, it was in a bright flame, and with a report peculiarly loud. It passed over the surface of the most highly rectified spirit of wine without firing it; but when I took too great a distance, the electric fire entered the spirit, and the whole plate was in a blaze in a moment.

I ONCE fancied that the fluidity of water was in a great measure the cause of this phenomenon; but I found I could not make it pass over the surface of quicksilver or melted lead; though neither of the rods with which the discharge was made touched the metals. A dark impression was made on the surfaces of both the quicksilver and the lead, of the usual size of the circular spot; and remained

mained very visible; notwithstanding the state of fusion in which the metals were.

So far was the electrical explosion from passing over the surface of any metal, that I observed, if the distance through the air, in order to a passage through the metal, was ever so little nearer than the distance along the surfaces, it never failed to enter the metal; so that its entering the surface of the metal, and its coming out again seemed to be made without the least obstruction. If as much water was laid on a smooth piece of brass as could lie upon it, it would not go over the surface of the water, but always struck through the water into the metal. But if the metal lay at any considerable depth under the water, it would prefer the surface. It even passed over three or four inches of the surface of water as it was boiling in a brass pot over the fire, in the midst of the steam and the bubbles, which seemed to be no hindrance to it.

ANIMAL fluids, of all kinds that I have tried, seemed in a peculiar manner to favour the passage of the electrical explosion over their surfaces, and the report of those explosions was manifestly louder than when water was used in the experiment. This I remarked more particularly when I made use of milk, the white and yolk of an egg, both fresh broken, and after it had stood a day or two, and had contracted a hard pellicle. In all the experiments with the egg, it was observed,

served, that no peculiar impression was made in the place where the electric matter first came upon the surface.

It was very remarkable, that the report made by all these explosions, in which the electric matter passed over the surfaces, was considerably louder than when the discharge was made between two pieces of metal; and they were observed by persons at some distance out of the house, and in a neighbouring house, very much to resemble the smart cracking of a whip; and indeed it would not be very easy to distinguish them. But the sound made by these explosions, though by far the loudest that I ever heard of the kind, fell much short of the report made by a single jar, of no very great size, of Mr. Rackstrow's; who says that it was as loud as that of a pistol.

It was pretty evident, that the distance at which the fire passed over animal substances was greater than it could be made on the surface of water; particularly in the first experiment of the leg of mutton. It also passed about ten inches over the surface of a piece of spinal marrow taken from an ox.

I was much struck with a beautiful appearance which occurred in the course of these experiments, though it was of a different nature from them. When the electrical explosion does not pass over the surface of water, but enters the fluid, it makes a regular star upon

it, consisting of ten or a dozen rays; and, what is most remarkable, those rays which stretch towards the brass rod that communicates with the outside of the battery are always longer than the rest; and if the explosion be made at such a distance, as to be very near taking the surface, those rays will be four or five times longer than the rest; and a line bounding the whole appearance will be a beautiful ellipsis, one of whose *foci* is perpendicularly under the brass knob with which the discharge is made.

It will be in vain to attempt these experiments without a considerable force. Nothing at all, to any purpose, can be done with a common jar; since the explosion of it will hardly pass over the surface of any conductor farther than it will discharge through the air. The charge of a jar containing three square feet of coated glass will not make any considerable appearance upon the water; and, as far as I can judge, the distance at which the explosion will pass along any surface is in proportion to the strength of the charge. For this reason I make no doubt but that I could have performed all the experiments above mentioned to much greater advantage, if I had applied a greater force, but that would have required more time, and a moderate force was sufficient to ascertain the facts.

N. B.

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N. B. IN these experiments, I put the discharging rod through a handle of baked wood; by which means, I could with safety lay one end of it upon the wires of the battery, and make the explosion with the other, on what substances I pleased.

**X 2      SECTION**

## SECTION XII.

## EXPERIMENTS ON THE TOURMALIN.

**F**ATIGUED with the incessant charging of the electrical battery, and sturned with the frequent report of its explosion, I was desirous of some respite from those labours, and with pleasure took up the gentle and silent TOURMALIN. And I make no doubt but that my readers, who must have sympathized with me, will be equally pleased with the change.

It was in the month of August 1766, that, being in London, I received from Dr. Heberden, who is glad to encourage every attempt in philosophical inquiries, his set of tourmalins; among which was that fine one which had passed through the hands of Mr. Wilson and Mr. Canton, and of which a description is given in the fifty-first volume of the Philosophical Transactions, p. 316. But notwithstanding I had this valuable stone so long in my possession, it was not till the latter end of December that I began to make any experiments with it, having, in the mean time, been engaged in other electrical pursuits. At length, however, having brought my other experiments to the state in which the

reader hath seen them, I was desirous of being an eye witness of the wonderful properties of this stone, and of pursuing a few hints which had occurred to me with respect to it. The result of my experiments I shall lay before the reader, after having informed him in what manner, and with what precautions they were made.

THE methods I used to apply heat to the tourmalin were various, but they will be sufficiently explained in the particular experiments. To ascertain the kind of electricity, I always had near me a *stand of baked wood*, from the top of which projected various arms for different purposes. Three of them were of glass, to two of which were fastened threads of silk, as it comes from the worm, supporting light pieces of down; from the other hung a fine thread, about nine or ten inches long; while a brass arm supported a pair of Mr. Canton's pith balls. At the other extremity of this arm, which was pointed, I could place a charged jar, to keep the balls constantly and equably diverging, with positive or negative electricity. Sometimes I suspended the balls, not insulated, within the influence of large charged jars. And lastly, I had always at hand *a fine thread of silk* not insulated, and hanging freely, to observe whether the stone was electrical or not when I began any experiments, and sometimes to measure the strength of the power which it had acquired.

BEFORE I began any experiments, I never failed to try how long my electrometers would retain electricity, and in what degree. If the thread would retain the virtue for a few minutes, I generally preferred it, when I wanted to communicate the electricity of the tourmalin, because it would catch it in a moment. If the thread would not retain the virtue long enough, or if I wanted a less variable degree of electricity than the thread could retain, I had recourse to the *feathers*, which never failed to retain the virtue that was communicated to them for several hours together. I have often found them pretty strongly electrified, after remaining untouched a whole night, though there had been no fire in the room. They might be touched without any sensible loss of their electricity; but they received the virtue very slowly.

THE reader must observe, that by the *positive* or *negative* side of a tourmalin, in the following experiments, I always mean the side which is positive or negative while the stone is cooling. Also, when I mention *the tourmalin* without any distinction, I always mean Dr. Heberden's large one, the convex side of which is positive in cooling, and the flat side negative.

THE consideration of Mr. Wilcke's experiments on the production of spontaneous electricity, by melting one substance within another, first made me conjecture, that the tourmalin



malin might collect its electricity from the neighbouring air. To ascertain this circumstance I made the following experiments, which seem to prove that my conjecture was just. It was with a view to this experiment that I first expressed a desire to have a tourmalin in my possession. I afterwards found that Mr. Wilson had made an experiment, mentioned vol. I. p. 372, which is, in part, favourable to this hypothesis, though he supposed the electricity to permeate the stone, so that one side might have been supplied from the other. But the following experiments will show, that the supposition of the permeability of the tourmalin to the electric fluid is altogether unnecessary to account for any of the appearances it exhibits.

On the standard bar of a most excellent pyrometer made by Mr. Ellicott, I laid a part of a pane of glass, and upon the glass Dr. Heberden's large tourmalin. The bar was heated by a spirit lamp placed underneath it; and I treated the tourmalin in this manner, to ascertain with exactness when the heat was increasing, decreasing, or stationary. In this disposition of my apparatus I observed, that, whenever I examined the tourmalin, the glass had acquired an electricity opposite to that of the side of the stone which had lain upon it, and equally strong. If, for instance, I presented the flat side of the stone to a feather electrified positively, as the heat was increasing, it would repel it at the distance of about two inches, and the glass would attract it at

the same, or a greater distance; and when the heat was decreasing, the stone would attract it, and the glass repel it at the distance of four or five inches. It made no difference which side of the glass I presented; both sides attracting or repelling the same feather with equal strength. When I fastened a shilling with sealing-wax upon the glass, the events were always the same. The electricity of both the shilling and the glass was always opposite to that of the stone. I was surprised to observe how soon the electricity, both of the stone and the glass, would change when it came to the turn; for in less than a minute I have sometimes found them the reverse of what they were before.

THERE was, however, in the cases in which I laid the convex side of the tourmalin upon the flat surface of the glass, or shilling, one exception to the rule above mentioned, viz. that, in cooling, the glass and shilling were positive, as well as the stone. This I imagined to be owing to the stone touching the surface on which it lay in so few points, that it collected its electricity from the air, and imparted it to the body on which it lay; and this supposition was confirmed by experiment. For getting a mold made for the convex side of the stone in plaister of Paris, and heating the tourmalin in the mold, fastened to a slip of glass, I always found the mold and the glass possessed of the electricity contrary to that of the stone, and equally strong. When they were cooling, the mold seemed  
some-

sometimes to be more strongly negative than the stone was positive; for, at one time, when the stone repelled the thread at the distance of about three inches, the mold attracted it at the distance of near six.

HAVING made the experiments above mentioned with the tourmalin placed upon glass, or conducting substances, laid upon the glass, I had the curiosity to try what would be the consequence of heating and cooling the stone in contact with other substances, both electrics and conductors. And these experiments brought me gradually to the discovery of a method of reversing all the experiments that have hitherto been made upon the tourmalin, making that side which is positive in heating or cooling to be negative, and that which is negative to be positive; so that the kind of electricity shall be just what the operator shall direct, by the application of proper substances to the stone.

I BEGAN these experiments with substituting another tourmalin instead of the piece of glass above mentioned; and when only one of the tourmalins was heated, they were both affected just as the tourmalin and glass had been. If, for instance, the negative side of a hot tourmalin was laid upon the negative side of a cold one, this latter became positive, as a piece of glass would have been in the same circumstances.

WHEN I heated both the tourmalins, though they were fastened together with cement, they both acquired the same power that they  
would

would have done in the open air. In these cases, as the stones could not be made to touch one another in a sufficient number of points, nothing could be concluded from the experiments. The same objection lay against heating or cooling the tourmalin upon rough glass; when I always found them both to be affected as they would have been if the glass had been smooth.

THIS consideration made me think of cooling the tourmalin in contact with *sealing-wax*, which might be made to fit the stone as exactly as possible, though it were ever so irregular. Accordingly I half buried the negative side of a tourmalin in hot sealing-wax; and when it was cold, turning it out of its waxen cell, found it positive (contrary to what it would have been in the open air) and the wax negative. The other side of the tourmalin, which was exposed to the open air, was affected in the same manner as it would have been if the opposite side had been exposed to the air too, so that both sides were positive in cooling. As the negative side of the tourmalin became positive by cooling in wax, I had no doubt but that the positive side would be so, as I actually found it.

I WOULD have ascertained the state of the different sides of the tourmalin when it was *heating* in wax, but I found it extremely difficult to do it with sufficient certainty. It cannot be known exactly when the stone begins to cool in these circumstances; besides, in this method of treatment, it must necessarily  
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ly be some time in the open air before it can be presented to the electrometer; and the electricity of the sides in heating is by no means so remarkable as it is in cooling. In the attempts I did make with the positive side of the tourmalin buried in wax, I generally found it negative, but once or twice it seemed to be positive.

WHEN I cooled the tourmalin in *quicksilver*, contained in a china cup, it always came out positive, and left the quicksilver negative; but this effect could not be concluded to be the consequence of the application of the one to the other, because it is almost impossible to touch quicksilver with the tourmalin without some degree of friction; which never fails to make both sides strongly positive though it be quite cold, and especially if the stone be dipped deep into it.

It then occurred to me, that the tourmalin would not be apt to receive any friction from simple pressure against the palm of my hand; and this being a conducting substance communicating with the earth, the circumstances of the experiment would be new, and might possibly produce new appearances. The event more than answered my expectations: for in heating or cooling the tourmalin in contact with the palm of my hand, each side of the stone was affected exactly in a manner contrary to what it would have been if exposed to the open air. In this case, though the positive appearances may be suspected to be ambiguous, on account of the difficulty of avoid-  
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ing some small degree of friction, in removing the stone from the hand; yet the negative appearances are, by that very circumstance, rendered the more indisputable, and therefore remove the objection from the positive ones. For the greater satisfaction of my reader, I shall relate these experiments exactly as they were made.

I FASTENED the convex side of Dr. Heberden's large tourmalin to the end of a stick of sealing-wax, and when it was quite cold, I pressed the flat side of it pretty hard against the softest part of the palm of my hand. Immediately upon this, presenting it to an electrified feather, it appeared to be strongly negative, contrary to what it would have been if exposed to the open air; and it continued negative till it had acquired all the heat it could get from my hand, when its power decreased, though it was sensibly negative to the last. Perceiving no alteration, I let the stone cool in the open air; when, according to Mr. Canton's rule, it grew more strongly negative, till it was quite cold. Thus the same side of the stone was made negative both in heating and cooling.

HEATING the same flat side, by holding it near a red-hot poker, and then just touching it with the palm of my hand (when I could not bear it to rest a moment) it became positive. Letting it cool in the air, it was negative, and touching it again with my hand it became positive. Thus I made the same side of the stone alternately positive and negative

tive for a considerable time; and at length, when I could bear to keep it upon my hand, it acquired a strong positive electricity, which continued till it was brought to the heat of my hand.

To complete these experiments, I removed the wax from the convex side, and fastened it to the flat side of the stone. Then warming the convex side, by pressing it against the palm of my hand, it became pretty strongly positive, contrary to what it would have been if heated in the open air, and continued positive in a small degree after it had got all the heat it could from my hand. Letting it cool in the open air, it grew, according to Mr. Canton's rule, more strongly positive, and continued so till it was quite cold. Thus the same side of the stone was made positive both in heating and cooling.

I THEN heated the convex side, by holding it near a red-hot poker, and pressing it against the palm of my hand, as soon as I could bear it, it became (contrary to what it would have been in the open air) pretty strongly negative; though it be extremely difficult to get a negative appearance from this side. It cannot always be caught when it is heating in the open air. Care, however, must be taken, lest a slight attraction of the electrified feather, by a body not electrified, be mistaken for negative electricity.

HAVING made the above mentioned experiments, to see how the tourmalin would be affected by being heated or cooled in contact

tact with various substances, to which only one of its sides was exposed at once; I made others in which the stone was entirely surrounded by them. It appeared very evident, from Mr. Canton's experiment, that it could answer no purpose to inclose it in substances that were conductors: for though the two electricities should be generated, the equilibrium would instantly be restored between them. I therefore made use of electric substances only, and began with *oil* and *tallow*, both covering the tourmalin with them when it was hot, and also heating it in boiling-oil. But this treatment produced no new appearance, the electricity of the stone being only a little lessened. The event was the same when a tourmalin was covered with *cement* made of bees wax and turpentine.

AT last I made a small tourmalin very hot, and dropping melted sealing-wax upon it, covered the stone all over, to the thickness of about a crown piece; and found it to act nearly, if not quite as well through this coating of wax, as if it had been exposed to the open air. I take it for granted, that the inside of the case of wax next to the stone was possessed of the electricity opposite to that of the stone, at the same time that the outside was the same with it. A pretty deception may be made by means of this experiment; for if a tourmalin be concealed in a stick of sealing-wax, the wax will seem to have acquired the properties of the tourmalin.

HEATING



HEATING the stone, or letting it cool *in vacuo* might easily be imagined to have the same effect as heating or cooling it in contact with conducting substances; I had the curiosity, however, to try the experiment, by letting it cool in an exhausted receiver, in which I had a contrivance to bring a thread of trial near it, or withdraw it at pleasure. The stone was set upright on its edge, by means of bits of glass which it touched but in a few points. The consequence was, that the virtue of the stone seemed to be diminished about one half; owing, perhaps, to the vacuum not being sufficiently perfect. For the same reason, the tourmalin has but little virtue immediately upon being taken out of boiling water, or after being heated in flame.

ONE time I fixed a thin piece of glass, with a small coating upon it, opposite and parallel to the flat side of the tourmalin, and at about a quarter of an inch distance from it, in an exhausted receiver; to observe whether the electricity would be transmitted from the glass to the stone through the vacuum: but though the glass was electrified, it was so slightly, that I could not be certain of what kind it was.

IN order to ascertain the circumstances relating to the change of the electricity of the tourmalin with more exactness than could be done by heating and cooling the stone in any of the usual methods, I laid it upon the standard bar of the pyrometer, and communicated  
heat

heat to it by a spirit lamp placed underneath it. The result of these experiments was in general agreeable to Mr. Canton's rules; but a few circumstances occurred in this method of treating it, which could not be determined in any other; and therefore it may be worth while just to mention them. I generally heated the bar, which is of iron, eight inches long, till the index moved seventy degrees, each of which corresponds to one 7200th part of an inch; and observed, that which ever side of the stone lay uppermost, it was extremely difficult to ascertain the nature of its electricity all the time the heat was increasing; though, in order to do it, I held over it an electrified thread, about two inches in length, fastened to a stick of sealing-wax, which just supported it in an horizontal situation. It was evident, however, that it was electrified, by its attracting a thread of trial at the distance of about a quarter of an inch; but if I took the stone off the bar, and immediately presented the side that had lain upon it to an electrified thread or feather, I always perceived the convex side to be negative, and the flat side positive in the same circumstances; but not half so much as they were in the contrary state by cooling. In this case, the two powers were very distinguishable by the small thread above mentioned, as the stone lay upon the bar; and also by bits of down fastened to silk threads. One of these, which had touched the convex side of the stone, as it lay uppermost upon the bar, could not

not be made to touch it again in less than five hours and a half.

To see what would be the effect of keeping the tourmalin in the very same degree of heat a considerable time together, I laid it upon the middle part of the bar, heated by two spirit lamps, one at each extremity, and making the index move forty-five degrees, I kept it in the same degree of heat, without the least sensible variation, for above half an hour together; and observed, that the upper side, which was the convex one, was always electrified to a small degree, attracting a fine thread at the distance of about a quarter of an inch. If in that time I took it off the bar ever so quick, and presented it to an electrified feather; the flat side, which lay upon the bar, was negative, and the upper side very slightly positive; as appeared by its only not attracting the feather. When I put a piece of glass betwixt the standard bar and the tourmalin, and kept them likewise in the same degree of heat, for the same space of time, the result was the same as before, and the glass was slightly electrified, in a kind opposite to that of the stone.

IN heating the tourmalin upon the pyrometer, one of its sides was necessarily made much hotter than the other. This inconvenience I avoided in the following method of treatment, which, though not so accurate in some respects, has peculiar advantages in others. By means of two rough places in the stone, I tied it in a silk thread, which only

touched the extreme edge of it on both sides. Being in this manner perfectly insulated, I contrived to make it hang in the air, at any distance from a fire, or candle, &c. and by twisting the string, I could make it present both its sides alternately, so as to heat it very equally.

WHEN, in this manner, I had made it so hot, that I could hardly bear to handle it, I let it remain in the same situation a quarter of an hour, in order to be sure that it was heated equally throughout. Then, with a bundle of fine thread, held some time before in the same degree of heat, I took off the electricity which the stone had acquired in heating, and continuing it in the same situation, I found it acquired extremely little, if any electricity. Sometimes, when I thought it had acquired a little (which might be occasioned by the variation of heat in the fire) it was so small, that I could not determine of what kind it was. This fully satisfied me of the justness of Mr. Canton's observation that it is not heat, but the circumstance of changing its degree of heat that gives electricity to this stone.

IF the stone be heated pretty suddenly, I have sometimes found that it may be handled, and pressed with the fingers several times before the electricity it acquires in heating will be changed, though it begins to cool the moment it is removed from the fire.

IN

IN this same method of treatment, I verified Mr. Canton's observation, that when the tourmalin is heated, and suffered to cool again, without either of its sides being touched, the same side will be positive or negative the whole time of the increase and decrease of the heat. But, as he observes, in his experiments on hot air, the stone must, in this case, be heated only to a small degree. I also proved the converse of this proposition; for, beginning where I left the stone in the last experiment, and removing it farther from the fire, both sides acquired a strong electricity, as usual; and bringing it again nearer to the fire, I observed that both the sides not only retained the electricity they had acquired in cooling, all the time it was heating, but a considerable time after it had remained in the same degree of heat.

I CANNOT, however, entirely acquiesce in the reason that Mr. Canton gives for this appearance: for if the surrounding air would conduct the electric fluid from the positive side of the stone to the negative, I should think it would be in the same situation as in the experiment Mr. Canton made upon it surrounded with water, and that neither side would discover any electricity at all. When the heat is three or four times greater than is sufficient to change the electricity of the two sides, the virtue of the stone is the strongest, and appears to be so when it is tried in the very neighbourhood of the fire. In the very

center of the fire, the stone never fails to cover itself with ashes, attracted to it from all sides, and from this property it acquired its name in Dutch.

It requires, indeed, some time for the electricity of the sides to change from one state to the other; and therefore the time of the sensible change is not always at the time of its beginning to cool, but these two circumstances will be brought nearer together the hotter the stone is made, because then the efforts (of whatever kind they are) to acquire any particular species of electricity will be the most vigorous, and sooner produce their effect; so as to be more able to overcome obstacles to it, such as must arise from the contrary electricity with which the stone is possessed. Thus, if either side of the stone be in a state to acquire either kind of electricity, and a quantity of the contrary electricity be communicated to it by friction or *abstracta*, that foreign electricity will be either only weakened, or lost, or changed; and these in a longer or a shorter space of time, according to the vigour, as we may say, with which the stone is made to exert itself to counteract that influence. But I have great reason to suspect my own opinion, when it is different from that of so accurate and excellent a judge of this subject as Mr. Canton.

It is a fact, however, that the stone often changes its electricity very slowly; and the elec-

electricity it acquires in cooling never fails to remain many hours upon it, with very little diminution. It is even possible that, in some cases, the electricity acquired by heating may be so strong, as to overpower that which is acquired by cooling; so that both sides may show the same power in the whole operation. And I am very certain that, in my hands, both the sides of Dr. Heberden's large tourmalin have frequently been positive for several hours together, without any appearance of either of them having been negative at all. Perhaps the flat side of this stone, which is positive in heating, might continue so according to Mr. Canton's observation, and the electricity of the convex side might have changed, as it very often does, too soon for me to observe it. This fact, however, has happened so often with me, and is so very remarkable, that I think I ought not to omit the mention of it, let the cause be what it will.

THIS appearance happened so constantly when I first began to make experiments with the tourmalin, that I had concluded the Duc de Noya had reason to assert, contrary to *Æpinus*, that both sides of the tourmalin in all cases acquired positive electricity; and I should have acquiesced in that opinion, had it not been for the friendly remonstrances of Dr. Franklin and Mr. Canton; in consequence of which I renewed my experiments, and at length found other appearances. At

the time above mentioned, I generally heated the tourmalin by presenting each side alternately to a red-hot poker, or a piece of hot glass held at the distance of about half an inch; and sometimes I held it in the focus of a burning mirror; but I have since found the same appearance when I have heated it in the middle of an iron hoop made red-hot. The stone, in all these cases, was fastened by its edge to a stick of sealing-wax. This appearance I have observed to happen the ofteneft when the iron hoop has been exceedingly hot, so that the outside of the stone must have been heated some time before the inside; and I also think there is the greatest chance of producing this appearance when the convex side of the stone is made the hotter of the two. When I heat the large tourmalin in this manner, I seldom fail to make both sides positive till the stone be about blood warm. I then generally observe a ragged part of the flat side, towards one end of the stone, will become negative first, and by degrees the rest of the flat side; but very often one part of the flat side will, in this method of treatment, be strongly positive half an hour after the other part is become negative.

THIS account of the appearance is made the more probable by the manner in which the stone was affected when only one of its sides was heated at one time. For when the convex side only was heated, the stone often  
con-



continued a long time with both its sides positive, generally till it was not sensibly warm. But, in this case, before the convex side became positive, it would sometimes be negative two or three minutes. On the other hand, when the flat side only was heated, it would be positive a long while, and the convex side negative; but the flat side becoming negative a considerable time before the convex side ceased to be so, both sides would continue negative till the stone was nearly cold.

EXTREMELY sorry I am for the article with which I must close this section. In the first of the above mentioned courses of experiments, that fine tourmalin, which has been so often mentioned in the course of this work, slipped out of my hands; and though it fell only from the height of my breast, upon a boarded floor, two pieces were broke off from one of its ends. The stone, however, is more disfigured than injured by the accident: for the larger of the fragments weighs but ten grains, and the smaller only one, while the rest of it weighs four penny weights sixteen grains. I cannot perceive that its virtue is at all lessened. Mr. Wilson observes, that there were several cracks in it; and for that reason I had been careful never to expose it to any great degree of heat.

It is broke with eight or ten different faces, each of which hath a most exquisite

site polish ; but there is no appearance of any *strata* or *laminæ* in the internal structure of the stone. A piece of glass or pitch might be supposed to break in the same manner. The larger of the fragments has considerable power, and the two sides have the same different powers that they had when they were part of the entire stone.

SECTION

## S E C T I O N XIII.

EXPERIMENTS IN WHICH RINGS, CONSISTING OF ALL THE PRISMATIC COLOURS, WERE MADE BY ELECTRICAL EXPLOSIONS ON THE SURFACES OF METALS.

**I**T was a discovery of Sir Isaac Newton, that the colours of bodies depend upon the thickness of the fine plates which compose their surfaces. He hath shown that a change of the thickness of these plates occasions a change in the colour of the body, rays of a different colour being thereby disposed to be transmitted through it; and, consequently, rays of a different colour reflected at the same place, so as to present an image of a different colour to the eye. A variation in the density occasions a variation in the colour, but still a medium of any density will exhibit all the colours, according to the thickness of it. These observations he confirmed by experiments on plates of air, water, and glass. He likewise mentions the colours which arise on polished steel by heating it, as likewise on bell metal, and some other metalline substances, when melted and poured on the ground, where they may cool in the open air, and he ascribes them to the  
*scorie,*

*scoriae* or vitrified parts of the metal, which he says most metals, when heated or melted, do continually protrude, and send out to their surfaces, covering them in the form of a thin glassy skin.

THIS great discovery concerning the colours of bodies, depending upon the thickness of the fine plates which compose their surfaces, of whatever density those plates may be, I have been so happy as to hit upon a method of illustrating and confirming by means of electrical explosions. A number of these being received on the surface of any piece of metal, change the colour of it to a considerable distance from the spot on which they were discharged, so that the whole circular space is divided into a number of concentric rings, each of which consists of all the prismatic colours, and perhaps as vivid as they can be produced in any method whatever.

It was not by any reasoning *a priori*, but by mere accident, that I was led to the discovery of these colours. Having occasion to take a great number of explosions, in order to ascertain the lateral force of them; I observed that a piece of brass, through which they were transmitted, was not only melted, and marked with a circle by a fusion round the central spot, but likewise tinged beyond the circular spot with a greenish colour, which I could not easily wipe out with my finger. Struck with this new appearance, I replaced the apparatus, and continued the explosions,  
till,

till, examining with a microscope, I plainly perceived all the prismatic colours, in the order of the rainbow. The diameter of the red, in this instance, was one third of an inch, and of the purple near one fourth. The diameter of the whole coloured space in the subsequent experiments, in which I generally used thirty or forty explosions, was near an inch.

PLEASED with the first experiment, I presently diversified it in a great variety of ways, the result of which I shall comprise in the following observations.

1. WHEN a pointed body is fixed opposite to a plain surface, the nearer it is placed, the sooner the colours appear, the closer do they succeed one another, and the less space they occupy. It seems, however, that when the point is at such a distance, that the electric matter has room to expand, and form as large a circular spot as the battery will admit, this coloured space is as large as it is capable of being made; but still the colours appear later, in proportion to the distance beyond that. When the point is fixed exceedingly near, or made to touch the surface, the colours appear at the first explosion, but they spread very irregularly, and make no distinct rings.

2. THE more acutely pointed is the wire, from which the electric fire issues, or at which it enters, the greater is the number of rings. A blunt point makes the rings larger,  
but

but fewer. It is likewise much later before they make their appearance at a given distance.

3. IN making these rings, the first appearance is a dusky red about the edge of the central spot; presently after which (generally after four or five strokes) there appears a circular space, visible only in an oblique position to the light, and looking like a shade on the metal. This expands very little during the whole course of the explosions. It seems to be an attempt, as it were, at the first red; for, by degrees, as the other colours fill the greater part of that space, the extreme edge of it becomes a deeper brown.

4. AFTER a few more explosions, a second circular space is marked, by another shade beyond the first, being one eighth or one tenth of an inch in breadth, which I have never observed to change its appearance, after ever so many explosions. This shade, by succeeding the first, which becomes gradually of a brown or light red colour, seems to be an attempt at the fainter colours, which intervene between the reds.

5. ALL the colours make their first appearance about the edges of the circular spot. More explosions make them expand towards the extremity of the space first marked out; while others succeed in their place; till, after thirty or forty explosions, three distinct rings appear, each consisting of all the colours. If  
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the explosions be continued farther, the colours become less beautiful and distinct, the red generally prevailing, and suffusing the rest.

6. THE innermost, *i. e.* the last formed colours, are always the most vivid, and those rings are likewise closer to one another than the rest.

7. THESE colours may be brushed with a feather, or a finger may be drawn over them, without injury; but they are easily peeled off, with one's nail, or any thing that is sharp. The innermost are the most difficult to erase.

8. THE first rings are sometimes covered with a quantity of black dust, part of which, however, may be wiped off, with a feather, and the colours will appear under it.

9. IT makes no difference, whether the electric matter issue from the pointed body upon the plate, or from the plate upon the pointed body, the surface opposite to the point being marked exactly alike in both cases; also the points themselves, from which the fire issues, or at which it enters, are coloured to a considerable degree, about half an inch. The colours, also, return here as upon the plate.

10. THE more circles are made at the same time, the more delicate, I think, the colours will be, whereas the surface is torn, as it were, by violent explosions, and the colours appear rough and coarse. But this roughness is only perceived on steel. On silver,  
6 tin,

tin, and polished brass, the colours were always equally free from that coarseness.

11. A POLISHED surface is not necessary for these colours, for they appear very well, though they do not make so beautiful an appearance on the rough surfaces.

12. THESE coloured rings appear equally well on all the metals that I have tried, viz. gold, silver, copper, brass, iron, steel, lead, and tin. I have not tried any of the semi-metals, but I have no doubt of their answering as well as the proper metals.

13. WHEN the pointed wire was made to incline to the plane on which the colours were made, the circular spot was quite round, and the center of it was in a perpendicular let fall from the point upon the plain surface; but the colours were projected in an oblong form, the center being in the pointed wire continued.

UPON shewing these coloured rings to Mr. Canton, I was agreeably surprized to find, that he had likewise produced all the prismatic colours from all the metals, but by a different operation. He extended fine wires, of all the metals, along the surface of pieces of glass, ivory, wood, &c. and when the wire was exploded, he always found them tinged with all the colours. They are not disposed in so regular and beautiful a manner as in the rings I produced, but they equally demonstrate, that none of the metals, thus exploded, discovers the least preference to one colour more than to another. A variety of other very extraordinary  
8 appear-



appearances occurred in the course of Mr. Canton's experiments in melting wires, but I forbear to mention them, as I hope he will soon favour the public with a communication of them himself.

IN what manner these colours are formed, it may not be easy to conjecture. In Mr. Canton's method of producing them, the metal, or the calcined and vitrified parts of it, seem to be dispersed in all directions from the place of explosion in the form of spheres, of a very great variety of sizes, tinged with all the variety of colours, and some of them smaller than can be distinctly seen by any magnifier. In my method of making these colours, they seem to be produced in a manner similar to the production of colours on steel, and other metals by heat, *i. e.* the surface is affected, without the parts of it being removed from their places, certain plates or *laminæ* being formed, of a thickness proper to exhibit the respective colours.

## SECTION

## SECTION XIV.

EXPERIMENTS ON THE LATERAL FORCE  
OF ELECTRICAL EXPLOSIONS.

**B**EING informed, in accounts of damages done by lightning, of persons and things being removed to a considerable distance, without receiving any hurt; I was excited to try whether I could produce similar effects by electricity. All the other known effects of lightning had been frequently imitated by the application of this power, but I do not know that this effect has ever been so much as taken notice of by any electricians. The experiments I presently found to be very easy, and I think it not difficult to ascertain the cause, and the manner in which this striking effect is produced.

IF pieces of cork, powder of any kind, or any light bodies whatever, be placed near the explosion of a jar or battery, they will not fail to be moved out of their places, upon the instant of the discharge. If the explosion of a large battery be made to pass over the surface of animal or vegetable substances, in the manner described above, and large corks be strewn along or near the part intended for it, it is surprizing to observe with what violence they

they will be driven about the room. This dispersion is in all directions from the center of the explosion, and it makes no difference whether the rods, between which it is made, be sharp pointed or otherwise.

THE effect of this lateral force is very remarkable in attempts to fire gun powder in electrical explosions. If the gun powder be confined ever so close in quills or cartridges, and they be held fast in vices; yet, when the explosion is made in the center of them, it will sometimes happen (even when a wire has been melted in the midst of the powder, and the fragments have been seen red-hot, for some time, in different parts of the room) that the powder has not been fired, or only a few grains of it, the rest being dispersed with great violence, part of it flying against the faces of persons who assisted in making the experiments. This circumstance, together with the charcoal being a conductor of electricity makes it so extremely difficult to fire gun powder by electrical explosions; and it is evidently owing to this lateral force, that parts of the melted wire fly so many ways, and to so great a distance from the place of explosion.

THIS lateral force is exerted, not only in the neighbourhood of an explosion, when it is made between pieces of metal in the open air, but also when it is transmitted through wires that are not thick enough to conduct it perfectly; and the smaller the wire, and the more complete the fusion, the greater is the

disperſion of light bodies placed near it. At one time, when the wire was not melted, but turned blue by the exploſion (in which caſe it generally aſſumes a duſky red, which laſts but for a moment) there was a ſmall diſperſion from every part of the wire, but by no means ſo great as it would have been if it had been melted, or only heated to a greater degree.

By a conſiderable number of trials, I found, that a greater force of exploſion would move light bodies at a greater diſtance, but the ſmaller the bodies were, the leſs was this difference; ſo that I imagined, that if they had no weight at all, they would, probably, be moved at the ſame diſtance, by the exploſion from any quantity of coated ſurface, charged equally high: but there was a great difference in the weight removed by different forces at the ſame diſtance. Placing the ſame piece of cork at the ſame diſtance from the place of exploſion, I found that the diſcharge of one jar removed it one fourth of an inch, two jars on inch and one fourth, three jars one inch and three fourths, and four about two inches, ſo that I do not wonder at very heavy bodies being moved from their places, and to conſiderable diſtances, by ſtrong ſaſhes of lightning.

THAT the immediate cauſe of this diſperſion of bodies in the neighbourhood of electrical exploſions is not their being ſuddenly charged with a quantity of electric matter, and therefore flying from others that are equally charged

charged with it is, I think, evident from the following experiments and observations. I never observed the least sensible attraction of these light bodies to the brass rods through which the explosion passed, or to the electric matter passing between them, previous to this repulsion, though I used several methods, which could not have failed to show it, if there had been any such thing. Sometimes I suspended them in fine silken strings, and observed that they had contracted no electricity after they had been agitated in the manner described above. Sometimes I dipped them in turpentine, and observed that no part of it was found sticking, either to the brass rods themselves, or to any part of the table betwixt them and the place where the light bodies had been laid. I even found that the explosion of a battery, made ever so near to a brass rod, did not so much as disturb the equilibrium of the electric fluid in the body itself; for when I had insulated the rod, and hung a pair of pith balls on the end opposite to that near which the explosion passed, I found that the balls were not in the least moved at the time of explosion; which they would have been, if part of the electric fluid natural to the body had been driven, though but for a moment, towards the opposite end. I also observed that the effect was the same when the explosion was made to pass through one of the knobs of the insulated rod. This lateral force was evident through thin substances of various kinds, interposed between

the explosion and the bodies removed by it ; as paper, tinfoil, and even glass ; for when some grains of gun powder were put into a thin phial close stopped, and held near the explosion of a battery, they were thrown into manifest agitation.

I THEREFORE think it most probable, that this lateral force is produced by the explosion of the air from the place where the explosion is made. For the electric matter makes a vacuum of air in its passage ; and this air, being displaced suddenly, gives a concussion to all the bodies that happened to be near it. Hence the removal of the light bodies, and the agitation communicated to the thin substances, and to the air, and the light bodies placed beyond them.

THE only objection to this hypothesis is, that this lateral force is not so much less in vacuo as might be expected, when the air is supposed to receive the concussion first, and to communicate it to other bodies ; but it must be considered, that the most perfect vacuum we can make with a pump is not free from air. I have tried to make this experiment in a Torricellian vacuum, but could not succeed at that time. Besides, as the electric matter of which an explosion consists must take a wider path in vacuo, if not equally fill the whole space, it may affect a body in its passage, without the intervention of any air. In condensed air, this latter force was not, as far as I could perceive, much encreased.

WILLING

## ELECTRICAL EXPLOSIONS. 341

WILLING to feel what kind of an impulse it was that acted upon bodies, when they were driven away by this lateral force of electricity, I held my finger near the path of an explosion of the battery, passing over the surface of a green leaf, when I felt a stroke, as of something pushing against my finger. Several corks, placed in the same situation, were driven to a considerable distance by the same explosion.

RECOLLECTING that this power, which I now call the *lateral force of electrical explosions*, must be the same with that which gives the concussion to water, mentioned in my experiments to imitate an earthquake, and to vegetable and animal substances, over the surface of which it passes; and being determined to make a more satisfactory trial of it than I had ventured to do before, I laid a green leaf upon the palm of my hand, intending to make the explosion pass over the leaf; but the leaf was burst, and torn to pieces, and the explosion passing over my hand gave it a violent jar, the effect of which remained in a kind of tingling for some time.

LASTLY, in order to judge the more perfectly of this force, I laid a chain communicating with the outside of the battery upon my bare arm above the wrist, and bringing the discharging rod near the flesh, within about two inches and an half of the chain, I made the explosion pass over that quantity of the surface of the skin. Had I taken a greater distance, I was aware that the explosion would

have entered the flesh, which, I was sensible, would have given a painful convulsion to the muscles through which it passed. In this case, the sensible effect was very different from that, being the same external *concussion* as before, and I have sometimes thought, that the sensation is not disagreeable. However the hairs upon the skin were all singed, and curled up along the whole path of the explosion, and for the space of about half an inch on each side of it also the *papillæ pyramidales* of the skin were raised, as when a person is shivering with cold. This was also the case in every part of the arm which the chain touched, and even that part of it which was not in the circuit. Both the path of the explosion, and the place on which the chain had lain, had a redness, which remained till the next day. Sometimes the flesh has contracted a blackness by this experiment, which has remained for a few hours.



## S E C T I O N XV.

VARIOUS EXPERIMENTS ON THE FORCE OF  
ELECTRICAL EXPLOSIONS.

**M**AKING the explosion of a battery pass over the surface of a green cabbage leaf, I observed that it left a track, near a quarter of an inch in breadth, exceedingly well defined, and distinguishable by a difference of colour from the rest of the leaf. Along this path also the firmness of texture in the leaf was entirely destroyed, that part becoming quite flexible, like a piece of cloth. Presently after it turned yellow, grew withered, and became perfectly brittle.

**W**ILLING to try the effect of this explosion passing along the surface of other substances, I laid a piece of common window glass on the path, pressed by a weight of six ounces; but it was shattered to pieces, and totally dispersed, together with the leaf on which it lay. Placing the black side of a piece of cork wood upon it, pressed by a weight of half a pound, the leaf was not rent, but the cork was furrowed all the way, a trench being made in it, about half an inch in breadth, and a quarter of an inch in depth. Laying the smooth cut surface of the piece of cork, it was furrowed all the way

as if it had been cut with a file, but not near so deep as before. Many of the small pieces which had been rubbed off in the explosion, remained in the furrow. Also the substance of the cork seemed to be shattered, and it was easily rubbed off, a little way into it.

I MADE this explosion on the surface of some red wine in a small dish, and kept a part of the same quantity exposed in a similar manner, but I could perceive no difference between them after several days.

THE track of an electrical explosion on the surface of the cabbage leaf being so well defined, suggested an experiment to ascertain whether there was any sensible *momentum* in the electric fluid, when it is rushing with violence from one side of a battery to the other. For this purpose I made the explosion pass over the leaves when they were cut in right and acute angles; so that the shortest path, from the inside to the outside of the battery, was to turn close at the angle; and observed, that it was not diverted from its course in the least degree by the rapidity of its own motion, but that it had turned exactly at the angle; and kept as close to the opposite side, as if the motion had begun at the angle. The electric matter had however been evidently attracted by the veins of the cabbage leaf, having pursued them a little way, at least having sensibly affected them, wherever it met with them in its passage.

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THIS experiment suggested another, intended to determine whether the force of an explosion was at all diminished by being diverted from a right lined course, and made to turn in a great number of angles. To do this, I first found, by a great number of trials, what length of a small iron wire I was able to melt with a battery of about twenty square feet, in the middle of a circuit of about three yards of brass wire, considerably thicker than the iron, and stretched in two right lines, suspended on silken strings. The length of the iron wire melted in these circumstances was about three inches. I then took the same brass wire, and, fixing pins into a board of baked wood, twisted it about them, making it turn in a very great number of acute angles ; and I put three inches of the same iron wire in the middle of this crooked circuit, that I had done in the straight one, so that the electric matter in the explosion was obliged to make a great number of turns at acute angles, before it could come to the iron wire ; but I always found that the same length of iron wire was melted in these circumstances, as in the other, and not the least difference was perceived in the force.

BUT though the *form* of the wire through which an explosion passed made no difference in its force, I found a very remarkable difference occasioned by the *length of the circuit*, in wires of the same thickness, and which, I own surprized me very much.

IN

IN order to ascertain the practicability of firing mines by electrical explosions, I took twenty-two yards of small brass wire (but so thick, however, that I could not have melted the least part of it, by the force of any battery I have ever constructed) and extending it along a dry boarded floor, with a small piece of iron wire, and a cartridge of gun powder about it, in the place that was most remote from the battery ; I found that, upon the discharge, the wire was not melted, nor the gun powder exploded ; also the report was very faint. In other circumstances a charge of the same battery was able to melt more than nine inches of this iron wire, and this same cartridge was easily fired near the battery, connected with shorter pieces of the same brass wire ; so that the diminution of force must have been owing to the length of the circuit.

IN the place of this small brass wire, I substituted an iron wire, one fifth of an inch thick ; when about half an inch of the small iron wire was exploded ; so that the force was not lessened so much in a circuit of the thick iron wire, as it had been in one of the small brass wire. In order to judge how much of the force might be lost by nearer circuits, consisting of less perfect conductors, I joined the middle of the circuit made by the iron wire with water, in which both the wires were immersed. The effect was, that the small iron wire was only made red-hot, but not exploded as before.

BEING

BEING sensible how much depended upon avoiding all lesser circuits, whereby part of the fire of an explosion might return to the battery, without reaching the extremity of the circuit, where I intended the whole of its force to be exerted ; in the remaining experiments, I insulated half the circuit of iron wire. There was no occasion for insulating the whole circuit ; for if there was but one passage *to* or *from* the middle of it, there could be put one *from* or *to* it. In this method it was easy to ascertain what loss of force was occasioned by the length of the circuit, as every other circumstance was carefully excluded. And it presently appeared to be very considerable ; for though I could melt nine inches of the small iron wire at the distance of fifteen yards from the battery, when I tried twenty yards, I found that I was just able to make six inches of it red-hot. The battery in these experiments was in the house, and the wires of which the circuit consisted were conveyed by silken strings into a garden adjoining to the house.

MENTIONING this loss of force, occasioned by the length of the circuit, in electrical explosions to Dr. Franklin, he told me that the same observation had occurred to him, and that he had also been disappointed in an attempt to fire gun powder at a distance from his battery.

STRUCK with this appearance, I endeavoured to ascertain the quantity of this obstruction, by trying what other courses the electric  
fire

fire would chuse, preferably to a long metallic circuit. In the first place, taking about a yard of the small brass wire mentioned above, I disposed it in the manner described [fig. 9. Pl. I.] connecting one of the ends with the outside of the battery and the other with the inside. In the first place, I brought the parts [a] and [b] (near the two extremities) into contact, and, upon the discharge, found there had been a fusion in that place, and that a great part of the fire had taken the shorter circuit, though it had been obliged to quit the wire in one place, and enter in again in another. Afterwards I removed the parts [a] and [b] to a small distance from one another, and, upon the explosion, observed a strong spark pass between them. Removing them to greater and greater distances, I found the explosion chose to pass above one third of an inch in the air, rather than make the circuit of the continued wire. Using a longer and smaller iron wire, the passage through the air exceeded half an inch. I then took four or five yards of iron wire, one tenth of an inch thick; when the passage through the air was still half an inch; and taking three yards and a half of a wire that was one fifth of an inch thick, the spark in the air was half an inch, and sometimes near three quarters of an inch. Making use of only half the length of this wire, the passage through the air was only half that distance, or one fourth of an inch. When I kept the place of near contact about the middle of this wire, and made the explosion

sion at the extremities of the whole wire, I was obliged to bring them about as near again, i. e. to little more than one eighth of an inch before the passage would be through the air ; so that the force of the whole explosion must have been greatly weakened by its passing through so much of the wire. Lastly, I took a pair of kitchen tongs, the legs of which were two feet in length, and the smallest part of them above half an inch in diameter ; when the circuit was made about one sixth of an inch in the air (for at that distance from one another the ends of the tongs had been fixed) rather than through four feet of that thick iron.

NOTWITHSTANDING this passage of the explosion through the air, at the same time that a metallic circuit was open for it, it was evident that the whole of the force did not pass this way, nor indeed the greatest part of it. For when I extended a small iron wire between [*a*] and [*b*], I could only make about half an inch of it red-hot, whereas, when there was no other metallic circuit, I was able, with the same battery, to explode more than two inches of it.

As the electric fire meets with so much obstruction in passing through a circuit of iron of this thickness, I make no doubt, but that it is considerably obstructed in passing through metallic circuits of any thickness whatever ; and that it would prefer a passage through the air, if they were made even of no great length. In this method, the different degrees of conducting

ducting power in different metals may be tried, using metallic circuits of the same length and thickness, and observing the difference of the passage through the air in each. N. B. A common jar answers as well in these experiments as a large battery.

IT is evident, from many experiments, that the whole fire of an explosion does not pass in the shortest and best circuit, but that if inferior circuits be open, part will pass in them at the same time. Of this I made the following satisfactory trial. I took an iron chain, and laid it upon a table, in contact with a charged jar; so that the parts of it made two circuits for the discharge, which I could vary at pleasure; and I observed that, when one of the circuits was but half an inch, and the other more than half a yard; yet, if the discharge was high, it always went in them both, there being considerable flashes between the links of the remotest part of the chain. If the charge was weak, it passed in the longer or metallic circuit only.

IT is evident that when the wires of a battery are not in close contact, there must be some loss of force in the discharge; but this never appeared to me to be so considerable, as Mr. L. Epinasse seems to have imagined\*. In order to ascertain this by experiment, I first found, by repeated trials, what length of an iron wire, of a certain thickness, I was able to melt with a battery consisting of twenty

\* Phil. Trans. Vol. lvii. pt. i. p. 186.



jars, constructed in the manner described above. It was about two inches and a half. I then soldered all the wires together, and also soldered one rod to them all, instead of a chain which I had used before, so that I avoided near a hundred sparks, in each of which some force had been lost; but I did not find, after many trials, that the power of the battery was sensibly diminished. I still could not melt three inches of the same wire.

## SECTION

## SECTION XVI.

## MISCELLANEOUS EXPERIMENTS.

I. OBSERVATIONS ON THE ELECTRIC SPARK  
TAKEN THROUGH SEVERAL PIECES OF  
METAL.

**M**ARCH the 24th, 1766. I observed that an electric spark taken from the prime conductor itself was not near so strong and pungent, as one taken through a piece of metal insulated, and interposed between my finger and the conductor.

THE effect was the same whatever was the form of the interposed piece of metal. And, in this manner, whatever was presented received a full and strong spark; whereas a great part is commonly dissipated, in pencils or stars, even when pretty large brass knobs are presented to the prime conductor itself, if the excitation be very powerful; unless both the conductor and knob have one precise degree of convexity, adapted to one another.

ONE single brass ball made the spark as strong as the interposition of a long piece of metal, or of many pieces.

I

WHETHER

WHETHER one, a few, or a great number of pieces were used, it seemed that the intervals taken together must be equal.

BUT these intervals taken together will be larger when the pieces are placed in a right line, than when they are laid in a curve.

WHETHER one body, or a number of them be interposed; if a spark be solicited it will not strike the first, unless it can, at the same time, strike all the rest.

ALL these experiments succeed, in the same manner, with the explosion of a charged jar.

SOME of the above mentioned circumstances, I afterwards found, had been taken notice of by Signior Beccaria.

## II. A DECEPTION RELATING TO THE DIRECTION OF THE ELECTRIC SPARK.

As I was once amusing myself with taking long sparks from a large prime conductor of polished copper, and considering the deceptions that electricians had fallen into with respect to the direction of the electric matter; I could not help being struck with one deception, which the evidence of my senses would never have rectified; and which showed very clearly, how little the evidence of the senses is to be depended upon in such cases. I observed, that, whether I made this large con-

ductor give, or take the electric fire (for I could make it do either at pleasure, and with the same force) I still fancied that a spark taken with a brass ball above the conductor descended to it, and that a spark taken below it ascended from it; but sparks taken laterally seemed to have no one certain direction.

### III. AN EXPERIMENT INTENDED TO ASCERTAIN WHETHER ELECTRIC SUBSTANCES, IN THEIR NATURAL STATE, CONTAIN MORE OF THE ELECTRIC FLUID THAN CONDUCTORS.

THINKING to ascertain Dr. Franklin's hypothesis, concerning the essential difference between conductors and non-conductors, I made a pretty large piece of glass red-hot (in which state I had proved it to be a real conductor of electricity) and placed it upon a smooth piece of copper, insulated; supposing that, if electric substances had naturally a much greater share of the electric fluid than conductors, this piece of glass, in passing from a conducting to a non-conducting state, must exhaust the copper of its natural share of the electric fluid, and leave it electrified negatively. But I could perceive no kind of electricity, either in the copper, or the glass, during the whole time of its cooling.

SOME

SOME time after, I found that Mr. Cigna had endeavoured to ascertain the same thing, by reducing ice into water; but ice and water are both conductors of electricity.

#### IV. THE MUSICAL TONE OF VARIOUS DISCHARGES ASCERTAINED.

As the course of my experiments has required a great variety of electrical explosions, I could not help observing a great variety in the musical tone made by the reports. This excited my curiosity to attempt to reduce this variation to some measure. Accordingly, November the 17th, by the help of a couple of spinets, and two persons who had good ears for music, I endeavoured to ascertain the tone of some electric explosions; and observed, that every discharge made several strings, particularly those that were chords to one another, to vibrate: but one note was always predominant, and sounded after the rest. As every explosion was repeated several times, and three of us separately took the same note, there remained no doubt but that the tone we fixed upon was, at least, very near the true one. The result was as follows.

A JAR containing half a square foot of coated glass sounded F sharp, concert pitch. Another jar of a different form, but equal surface, sounded the same.

A a 2

A JAR

A JAR of three square feet sounded C, below F sharp. A battery, consisting of sixty-four jars, each containing half a square foot, sounded F below the C.

THE same battery, in conjunction with another of thirty-one jars, each containing a square foot, sounded C sharp. So that a greater quantity of coated glass, always gave a deeper note.

DIFFERENCES in the degree of a charge in the same jar made little or no difference in the tone of the explosion ; if any, a higher charge gave rather a deeper note.

FROM these experiments it will be easy for any person to compare the quantity of square feet of coated glass, with the lengths of musical strings giving the same note. For this purpose, I could easily have found more terms of the series ; but I am afraid philosophers in general will think it trifling enough to have found so many. I do not expect that electrical explosions will ever be introduced into concerts of music ; or that these experiments will be of any use to measure the extent of the clouds from which a clap of thunder proceeds. But true philosophers will not absolutely despise any new fact or observation, though it have no immediate, or apparent use.

V. EXPERIMENTS ON THE EFFECTS OF  
GIVING A METALLIC TINGE TO THE  
SURFACE OF GLASS.

IT has long been a question among electricians, where the electric matter that constitutes the charge of a plate of glass lies; whether within the pores of the glass, or only upon the surface; and some experiments I have made will perhaps be thought to throw some light on this difficult subject.

I CONSIDERED that the common coating of a jar is not in actual contact with the glass, but that the metallic tinge, which is given to glass by an electric explosion of the metal upon its surface, is probably in contact with it, if not lodged in its pores. I therefore gave a coating of this kind to both sides of a plate of glass; and at first imagined that the glass coated in this manner did receive a charge, as well as if it had been coated in the common way; for it gave a real shock; but I very well remember, at that time, being a little surprised to see the electric fire run over the surface of that coating, a thing not possible in the common way. However, not sufficiently attending to that circumstance, I was pursuing the experiment, and trying whether, by combining this piece of glass with a large battery, and making the discharge of both upon this metallic tinge, I

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could

could not melt part of it, and thereby fetch it out of the glass; as that method would have melted, and absolutely dispersed a considerable part of the coating of a common jar. But I was prodigiously surprised to find, that, though the connection of this metallic tinge with the battery was complete, the discharge could not be made by bringing the discharging rod upon it; though within three quarters of an inch of another brass rod, that formed the communication between this plate of glass and the battery. This convinced me that the metallic tinge did not answer the purpose of a coating; and I presently satisfied myself, that a piece of uncoated glass would receive just such a charge as the tinged glass had done.

To ascertain this matter still farther, I struck a tinge of this kind along two opposite sides of a glass tube, about half a yard in length; and holding it with my hand in contact with a part of this metallic tinge, found that it was excited just like another tube: for when I discharged the electricity of any part of the tube where the tinge was struck, it did not at all discharge other parts of the tube, whither the same tinge extended. Also the electric snapping from the tinged part of the glass could not be distinguished from the snapping at other places; except that, sometimes, where the gold lay thicker than ordinary, a denser stream of electric matter was visible on its surface, and ran in several small streaks, in  
different



different directions, from the place where the spark was taken.

THIS experiment seems to show, that a coating of metal exceedingly near the surface of the glass is not at all affected either by the excitation or charging of it; and seems to confirm the hypothesis of the electric fluid not entering the pores of the glass.

As the giving this metallic tinge to both sides of a plate of glass is not very easy, the reader will not, perhaps, be displeased to be informed in what manner I succeeded in it. After fatiguing myself a long time in endeavouring to strike a piece of leaf brass into the two sides of a plate of glass, to serve instead of a coating (having always broken the glass in fixing either the first or second coating) I at length put two other pieces of glass, one on each side of that to which I intended to give the tinge, with pieces of leaf brass between them both; and making one explosion through both of them at the same time, the upper and the lower piece of glass were shattered to pieces, but the middle piece (being equally affected on both sides) remained whole, and the coatings were nearly as I wished them.

VI. AN EXPERIMENT INTENDED TO ASCERTAIN WHETHER FERMENTATION CONTRIBUTES TO THE PRODUCTION OF ELECTRICITY.

SEPTEMBER the 3d. In order to determine whether any of the electric fluid was discharged from, or acquired by bodies in a state of *fermentation*; I hung a pair of pith balls at the extremity of a piece of wire communicating with a quantity of steel filings, fermenting with oil of vitriol, inclosed in a glass vessel. But they never separated in the least.

VII. AN EXPERIMENT INTENDED TO ASCERTAIN WHETHER EVAPORATION CONTRIBUTES TO THE PRODUCTION OF ELECTRICITY.

DECEMBER the 26th. I put a small quantity of water upon a thin piece of glass, and made it all suddenly evaporate by a red-hot iron held under it; but the glass had acquired no degree of electricity. The weather was frosty.

VIII. AN

VIII. AN EXPERIMENT INTENDED TO ASCERTAIN WHETHER FREEZING BE ACCELERATED OR RETARDED BY ELECTRIFICATION.

JANUARY the 6th, 1767. I exposed two dishes of water in the open air, while it was freezing intensely, and electrified one of them pretty strongly; but could perceive no difference in the time, either when it began to freeze, which was in about three minutes, or in the thickness of the ice when both had been frozen some time.

HAPPENING to cast my eyes into the fields, out of the window, through which I had put the board which I used for the purpose of this experiment, I observed, on each side of the electrified wire, the same *dancing vapour*, which is seen near the surface of the earth in a hot summer's day, or near any heated body that occasions an exhalation of vapours.

IX. THE EXAMINATION OF A GLASS TUBE, WHICH HAD BEEN A LONG TIME CHARGED AND HERMETICALLY SEALED.

DECEMBER the 30th. I examined a glass tube, about three feet in length, one half of which I had charged in the month of March preceding, and then sealed hermetically; but could not perceive that it was excited in the least

least degree, either by heating or cooling. The difference in the result of this experiment from several of Mr. Canton's, related vol. I. p. 346, I attribute to the thickness of the glass of my tube. Mr. Canton charged small balls exceedingly thin. I also observed that there was no perceivable difference in the excitation of the charged or uncharged part of this tube, and that both parts acted exceeding well.

I AFTERWARDS opened this tube, and pouring a quantity of leaden shot into it, found it to contain a very good charge. It gave me one considerable shock, and several small ones; as I made no use of an outward coating, but only discharged it by grasping it in several places by my hand.

#### X. THE WEIGHT REQUISITE TO BRING SOME BODIES INTO CONTACT ASCERTAINED BY THE ELECTRICAL EXPLOSION.

It is plain from optical experiments, and also from a variety of other considerations, that bodies of no great weight lying upon one another, are not in actual contact. As the same thing is demonstrated by an electric spark being visible between pieces of metal lying upon one another, and other effects of electricity (particularly the fusion of the parts through which it goes out of one body and enters another, not actually in contact

with it) I was desirous to determine, by this criterion, what weight was sufficient to bring bodies into actual contact. With these views, I began with laying twenty smooth shillings upon one another, and making the discharge of the battery through them; thinking that the fusion would disappear, when the weight was sufficient to press them into contact. But I found that the whole column was not sufficient; for every piece was melted on both its sides, so that every two contiguous sides had spots exactly corresponding to one another. The deepest impressions were made near the top of the column, but they did not diminish with exact regularity. Perhaps small particles of dust might prevent some of them from coming sufficiently near one another.

AFTERWARDS, I gradually increased my weights, till I found that about six pounds was sufficient for my purpose. The fusion was visible under that weight, but never under above half a pound more, though I repeated the experiment several times.

I HAD some suspicion, that the largeness of the explosion might have occasioned a momentary repulsion, separation, and consequent fusion of these pieces of metal, though pressed by such a weight, but I found I was not able to produce any fusion; under a greater weight than that above mentioned, though, instead of thirty-two square feet of coated glass, I used above sixty.

# XI. THE EFFECT OF THE ELECTRICAL EXPLOSION TRANSMITTED THROUGH VARIOUS LIQUORS.

I BELIEVE it is generally supposed, that ale, and other liquors are turned sour by lightning, and I was desirous of ascertaining whether that fact (if it be one) was owing to the liquors being properly struck with the lightning, or to the state of the air, &c. during the thunder storm. In order to this, I provided myself with a glass tube, nine inches long, and about a quarter of an inch in diameter, and by inserting a wire into one end of it, which was stopped with sealing-wax, could easily transmit an electrical shock through any substances contained in it.

By this means, November the 13th, I began with discharging the explosion of the battery through this tube, filled with *fresh small beer*, and observed a considerable quantity of fixed air, or something in the form of bubbles, to ascend in it; but when I tasted it, I could perceive no difference between it and that out of which it was taken. No doubt the escape of so much air would tend to make it grow stale something sooner.

I THEN discharged several large shocks through a tube filled with *red wine*, but, after two or three days, could perceive no alteration in its taste, or other sensible qualities. In this discharge, the electric matter did not  
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immediately strike the wine, but a metal rod; which just touched its surface; but I afterwards gave it two or three more shocks, in which the wine itself was made to receive the explosion, but there was no variation in the effects.

I PASSED the shock through a tube filled with *milk*, in both the methods above mentioned; but it was sweet three days after. Also a tube filled with *fresh ale* received several large shocks without undergoing any sensible change of properties.

IN all these explosions I held the tube in my hand, without feeling any thing of the shock.

I ALSO made the electric spark visible a great number of times in a small quantity of *syrup of violets*, without producing any change of colour, or other sensible qualities.

## XII. OBSERVATIONS ON THE COLOURS OF ELECTRIC LIGHT.

FINDING it advanced in the writings of several electricians (who must have copied it from one another, without ever repeating the experiment, though it may be done so soon) that electric light contained no prismatic colours; I had the curiosity to try so extraordinary a fact, and immediately saw both the fallacy of the experiment when it was first made, and the cause of it. Holding a prism before my eyes, while the electric sparks were

were taken at the prime conductor, I observed as beautiful prismatic colours as any that are exhibited by the image of the sun; but when the light was a little diffused, as in those red or purple parts of a long spark, as it is called, the colours were not so vivid, and less easily distinguished from one another; and when the light was still more diffused, through a vacuum, the prism made no sensible alteration in the appearance of it. Thus the middle part of any large object appears of its natural colour through a prism: for though the rays be really separated, they are immediately confounded with others from different parts of the same object; so that its natural colour must necessarily be the result.

As the flames of different bodies yield very different proportions of the prismatic colours, I have often thought of attempting to ascertain the proportion of these colours in electric light, and compare it with the proportion of colours from light procured in various other ways; but I have not had leisure to pursue the inquiry.

THE electric spark, taken in the middle of a phial filled with inflammable air, is always of a red or purple colour, and cannot be made to look white; but the larger the explosion is, the nearer it approaches to white.

I SHALL close this article with just mentioning another deception, which some persons may possibly lie under, with respect to what is called the *length of the electric spark*. When a jar is discharged, it may be imagined, that a body of fire is seen extending from  
the



the inside to the outside; whereas it is pretty certain, that that appearance is occasioned by the very rapid motion of a single ball of fire; in the same manner as a lighted torch, with no greater motion than a man's arm can give to it, will seem to make an entire circle of fire. That the fire of an electrical explosion consists of a ball, or cylinder, of no great length, seems pretty evident from one of the experiments with the circles, in which the diameter of the circle was the same, whether the explosion was taken at the distance of half an inch, or of two inches; and also from the experiments of its passage over surfaces, in which it was sometimes made twenty times longer than usual, without any sensible diminution of its thickness.

### XIII. OBSERVATIONS ON THE SMALL WIRES THAT COLLECT ELECTRICITY FROM THE EXCITED GLOBE.

HAVING made use of several brass wires, about two inches and a half long, to collect the electric matter from my globe; I observed, after a month or two, that about half an inch of the ends of them, which touched the globe, had contracted a blackness, particularly on the side which lay next the globe. I then took them off the ring to which they had hung, and rubbing them carefully, observed, that the same friction which made the rest of the wire quite bright, made but little alteration in this acquired blackness. Recollecting, at the

the same time, S. Beccaria's theory of magnetism, instead of replacing the wires, I hung two very fine needles in their place; and December the 20th, after about two months, in which I had made the most use of the machine, I examined them, and found that blackness at their points, but could not be sure that they had acquired any degree of magnetism. They had, indeed, a very small degree; but I had not examined them so very accurately before I hung them on, as I did afterwards. The experiment deserves to be repeated with more care, but it requires a longer and more constant use of an electrical machine than, it is probable, I shall ever have an opportunity of employing.

#### XIV. EXPERIMENTS INTENDED TO ASCERTAIN THE DIFFERENCE IN THE CONDUCTING POWER OF DIFFERENT METALS.

IN a conversation I once had with Dr. Franklin, Mr. Canton, and Dr. Price, I remember asking whether it was probable that there was any difference in the conducting power of different metals; and if there was, whether it was possible to ascertain that difference? I have since endeavoured to carry into execution a scheme proposed by Dr. Franklin, viz. transmitting the same explosion of the battery through two wires at the same time, of two different metals, and of the same  
6 thick-

thickness. They were hooked one to the other, and held fast in hand vices, after they were measured with a pair of compasses to exactly the same length. The experiments were much more pleasing and satisfactory than I expected, but the result by no means corresponded to my ideas *a priori*.

I FIRST joined a piece of iron wire, and a piece of copper wire. The explosion totally dispersed the iron, and left the copper untouched. The brass likewise disappeared when joined with the copper, and the iron when joined with the brass.

So far the experiments were extremely easy; a single charge of the battery sufficing to determine the difference between any two; but when I came to compare the more perfect metals, I found much more difficulty, and was obliged to try four or five charges of the battery upon every two: for, their conducting powers being nearly the same, I either made the charge too high, and dispersed them both; or too low, and touched neither of them. At length, I happened to hit upon such charges, that the copper vanished, and left both the silver and the gold; and the gold remained when the silver was dispersed. The hook, however, of the silver was melted off when the copper was dispersed, and the hook of the gold when the silver was dispersed: for the heat is always the greatest where the electric fire passes from one body to another. Before the dispersion both of the copper and the silver, I had

made explosions of such a strength, as though too small to melt them, gave them a blueish tinge.

FROM these experiments it is easy to settle the order in which the metals above mentioned are to be ranked, with respect to the power of electricity to melt them. It is as follows.  
*Iron, brass, copper, silver, gold.*

NOT being able to get *lead* or *tin* drawn into wires, I got pieces of those metals rolled into plates equally thin, and taking small slips, of equal length and breadth, I transmitted the explosion through them; when the lead gave way the first. I intended to have compared these plates with others of iron, brass, &c. but had not an opportunity. I have little doubt but that tin would melt before iron; though indeed I had expected that tin would have melted before lead, and gold before silver. But according to Mr. Wilcke's experiments, lead is a worse conductor than any of the other metals. My own experiments on the circular spots, made me expect that gold would have melted before silver.

IT is very remarkable, that when iron wire is melted by the electric explosion, *bright sparks* are generally dispersed about the room, in all directions; but that they are seldom, or never seen when wire of any other metal is used. If but a small residuum of a battery be taken between two iron rods, when the explosion is extremely little, a great number of small sparks will fly in all directions from the iron, to the distance of about an inch, and exhibit

exhibit a beautiful appearance. Fewer of these sparks will be seen if one of the rods be brass, and, I think, none in these small discharges, if they both be brass.

BEFORE any use can be made of these experiments, to determine the relative conducting power of the several metals, the order in which they melt with common heat should be compared with the order in which they melt with the electrical explosion; and the French translation of this history places them in the following order; tin, lead, silver, gold, brass, copper, iron. It is remarkable that iron should require more heat to bring it into a state of fusion than any other metal, and yet should require but a small force of electricity to do it. Before this matter can be settled, it should likewise be found, how much more easily any of the metals will be melted before another, by transmitting shocks through wires of different lengths and thicknesses, which would be a very tedious business. I make no doubt but that an explosion which melts a copper wire of any given diameter would disperse an iron wire of twice the diameter, so that copper would be a much greater security, as a conductor to guard a building from lightning than iron, besides its being less liable to rust; but then it is more expensive.

## XV. EXPERIMENTS WITH AN ELECTRIFIED CUP.

I SHALL close the account of my experiments with a small set, in which, as well as in the last, I have little to boast besides the honour of following the instructions of Dr. Franklin. He informed me, that he had found cork balls to be wholly unaffected by the electricity of a metal cup, within which they were held; and he desired me to repeat and ascertain the fact, giving me leave to make it public.

ACCORDINGLY, December the 21<sup>st</sup>. I electrified a tin quart vessel, standing upon a stool of baked wood; and observed, that a pair of pith balls, insulated by being fastened to the end of a stick of glass, and hanging intirely within the cup, so that no part of the threads were above the mouth of it, remained just where they were placed, without being in the least affected by the electricity; but that, if a finger, or any conducting substance communicating with the earth, touched them, or was even presented towards them, near the mouth of the cup, they immediately separated, being attracted to the sides; as they also were in raising them up, the moment that the threads appeared above the mouth of the cup.

If the balls had hung in the cup a considerable time without touching it, and they  
were

were taken out immediately after the electricity of the cup was discharged, they were found to have acquired no degree of electricity.

If they had touched any part of the cup, though they showed no electricity while they were within it; yet, upon being taken out, they appeared to have acquired some; which was more if they had touched a part near the edge of the cup, less if they had touched any part more remote from the edge, and least of all if they had touched the bottom only. If they had first touched the side near the top, and then the bottom, they came out with that small degree of electricity which they would have acquired, if they had touched the bottom only.

IN any case, if the balls were taken out while the cup remained electrified, they necessarily acquired some degree of electricity, in passing the mouth of the cup.

To pursue this experiment a little farther, I took a small coated phial, such as is represented upon the stool [*c* Pl. II.] and observed, that when I held it by the wire, within the electrified cup, it acquired no charge, the electricity of the cup affecting both the inside and outside coating alike. If the external coating touched the bottom of the cup, the phial received a very small charge. If it was made to touch the side, it acquired a greater charge; and the nearer to the top it was held,

held, the higher charge it received; the wire of the phial, which communicated with the inside coating, being farther removed from the influence of the electricity of the cup.

MAY we not infer from this experiment, that the attraction of electricity is subject to the same laws with that of gravitation, and is therefore according to the squares of the distances; since it is easily demonstrated, that were the earth in the form of a shell, a body in the inside of it would not be attracted to one side more than another.

DOTH it not follow from the experiments of the balls, compared with those with the phial, that no body can receive electricity in one place, unless an opportunity be given for its parting with it in another; at least, that a quantity must be repelled from any particular part before any more can enter, since a small body can no more receive electricity when all its sides are equally exposed to the action of an electrified body, than a phial can be charged when both its coatings are equally exposed to the same electricity?

Do not these experiments, likewise, favour the hypothesis of S. Beccaria, that there is no electrical attraction without a communication of electricity?

MR. LULLIN also made several of these experiments with an electrified hollow vessel.



fel. He observed, that if an electrified cork ball, suspended by a silken string, was let down into the vessel, and touched the bottom, it left all its electricity behind it. He also made the experiment with a glass vessel, coated at the bottom and charged, with the same event. It also made no difference whatever was the form, or size of the body let down into it, provided it was one third less than the depth of the vessel; but if, when it touched the bottom, it, at the same time, reached the top, or came near the top of the vessel, it acquired electricity; and a considerable degree, if it exceeded the top. The form of the vessel made no difference in his experiments, nor did it make any whether the vessel was intire or perforated. A wire net answered perfectly well. These experiments, Mr. Lullin imagines, clearly prove Nollet's doctrine of the constant motion of electric atmospheres: for he thinks, that this free motion on which electrification depends, is prevented from taking place within the vessel, by the contrary tendency of the opposite sides\*.

DR. FRANKLIN, in the last edition of his Letters, p. 326, says, that possibly the mutual repulsion of the opposite sides of the

\* *Dissertatio physica*, p. 38.

electrified

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electrified cup, may prevent the accumulation of the electric atmosphere upon them, and occasion it to stand chiefly on the outside. But he recommends it to the farther examination of the curious.

A CATA-

A CATALOGUE OF BOOKS WRITTEN ON THE SUBJECT OF  
ELECTRICITY, EXCLUSIVE OF PAPERS IN BOOKS OF PHI-  
LOSOPHICAL TRANSACTIONS, AND OTHER MISCELLANE-  
OUS WORKS; DISTINGUISHING [BY ASTERISMS] THOSE  
WHICH THE AUTHOR HAD SEEN; AND MADE USE OF IN  
COMPILING THIS WORK.

- \* **G**ILBERT de Magnete Magneticisque corporibus, 1600,  
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- \* *Ottò de Guericke's* experimenta nova Magdeburgica,  
1672, Amsterdam, folio.
- Haukeſbee's* phyſico mechanical experiments, 1709, 1719,  
London, octavo.
- G. M. Boze's* oratio inauguralis de electricitate, 2 parts, 1738,  
Wittemburg, Gralath's bibliothek.
- J. Mortenſon's* diſſertatio de electricitate, 1740, 1742, Upſal,  
quarto, Gralath's electritche bibliothek.
- \* *Deſaguliers's* diſſertation concerning electricity, 1742, London.
- C. A. Hauſenii* novi proſectus in hiſtoria electricitatis, Leiſpſe,  
1743.
- C. G. Kratzenſtein's* abhandlung von der nutzen der electricitat  
in der artzney wiſſenſchaft, 2d. edit. 1745,  
Halle. Gralath's bibliothek.
- A. G. Roſenberg's* verſuch einer erklarung von den urſachen der  
electricitat, 1745. Breſlau. Gralath's biblio-  
thek.
- Nelle's* conjeſtures ſur les cauſes de l' electricité des corps,  
Paris, 1745. Wiſſon, p 12.
- eſſai ſur l' electricité des corps, 1746, 1754. Paris.
- \* *Waitz's* abhandlung von der electricitat und deren urſachen,  
together with two other eſſays in High Dutch. and one  
in French, all written for the prize propoſed by the a-  
cademy at Berlin, 1745, Berlin, quarto.
- \* *Wiſſon's* eſſay towards an explication of the phenomena of  
electricity deduced from the ether of Sir Iſaac Newton,  
1746, London, octavo.
- \* *Watſon's* experiments and obſervations, tending to illuſtrate  
the nature and properties of electricity, 3d edit. 1746,  
London, octavo.
- \* ——— ſequel to the experiments and obſervations, tending  
to illuſtrate the nature and properties of electricity,  
1746, 1747. London, octavo.
- \* *Frike's* eſſay to ſhow the cauſe of electricity, 1746, London,  
octavo.
- Kratzenſtein's* theoria electricitatis, more geometrico explicata,  
Hall, 1746.
- J. Piderit's* diſſertatio inauguralis de electricitate, 1745, Mar-  
burgh. Gralath's bibliothek.
- \* *Martin's* eſſay on electricity, 1748, Bath. octavo.
- Boze's* recherches ſur la cauſe, et ſur la véritable theorie de l'  
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Conjetture fifiche intorna i fenomeni della machina elettrica, Rome, 1746, octavo. Hist. p. 168.

*Francisco Pivatti's* lettere della ellettricità medica, Venice, 1747.  
*Boze's* tentamina elettrica, Wittemberg, 1747.

\* *Giambattista Favre's* conjetture fifiche intorno alle cagioni de fenomeni osservati in Rome nella machina elettrica, 1747, Rome, quarto.

\* *Rackstrow's* miscellaneous observations, together with a collection of experiments on electricity, 1748, London, octavo.

\* *Watson's* account of experiments, made to discover whether the electric power would be sensible at great distances, &c. 1748, London, octavo.

Recueil des traités sur l'électricité, traduits de l'Alemand et de l'Anglois, Paris, 1748, octavo, 3 vols.

\* *Nollet's* recherches sur les causes particulieres des phenomenes électriques, 1749, Paris, twelves.

\* *Jallabert's* experiences sur l'électricité avec quelques conjectures sur la cause de ses effets, 1749, Paris, twelves.

A Latin dissertation on electricity by *P. Francois Plata*, a jesuit of Palermo, 1749. Histoire.

\* *Wilson's* treatise on electricity, 1750, 1752, London, octavo. N. B. *This book is quoted in this work by the title of Wilson's Essay.*

\* *Secondat's* histoire d'électricité in his *Observations Physiques*, 1750, Paris, twelves.

\* *Boulanger's* traité de la cause et des phenomenes de l'électricité 1750, Paris, twelves. N. B. *I have seen no more than one part of this treatise, which I had of Dr. Watson. How many more parts there are of the whole work, I have not been informed.*

*Jos. Veratti's* observations sur l'électricité, aux quelles on a joint les experiences faites a Montpellier pour guerir les paralytiques au moyen de l'électricité, a la Haye, 1750, twelves.

*Electricorum effectuum explicatio*, by *Father Bini*, 1751.

\* *Histoire generale et particuliere de l'électricité*, 1752, twelves.

*J. H. Winckler's* progr. de avertendi fulminis artificio ex doctrina electricitatis, Leipzig, 1753.

\* *Giambattista Biccaria* dell' elettricismo artificiale e naturale, 1753, Turin, quarto.

*S. H. Quetmalz's* dissertatio de viribus electricis medicis, Leipzig, 1753, quarto.

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part. 3. 1754, Lond.

quarto. \* *Franklin's*

- *Franklin's new experiments and observations on electricity, made at Philadelphia in America, new edition with additions, 1769.*
- Brevis relatio de electricitate propria lignorum, authore P. Windelipo Amm *rsen* De Lucerne Helvetiorum. Ord. Minim St. Francisci de P. Conventual. 1755. Nolle's letters, vol. 2. p. 235.
- J. B. Landriani dissertatio de nova electricitatis theoria. Milan, 1755. Wilcke, 12.
- J. A. Euleri, disquisitio de causa physica electricitatis, ab academia scientiarum imperiali Petropolitana præmio coronata, Petersburg, 1755. Wilcke, 12.
- *Dalibard's histoire abrégée de l'electricité, and French version of Franklin's letters, 1766, Paris, 2 vols. twelves.*
- *Lover's subtle medium proved, 1756, London, octavo.*
- *Daniel Grolath's geschichte der electricität in the memoirs of the society at Dantzick, 1747, 1754, and 1756, Dantzic, quarto. N. B. Vol. 1st. 2d. and 3d. of this work, in the marginal references mean the volumes of the work in which it is contained.*
- *Electrische bibliothek. in the Dantzick memoirs for 1754, and 1756.*
- *Johannes Carolus Wilcke's disputatio physica experimentalis de electricitatibus contrariis, 1757, Rostock, quarto. N. B. This admirable treatise consists of four parts, but, to my great regret, the copy which I had contained only the three first of them. It was that which the author sent to Dr. Franklin, before the remainder was printed.*
- Butschany dissertatio de fulgure et tonitru ex phenomenis electricis. Gottingen, 1757, quarto.*
- *Giambattista Beccaria's lettere dell' elettricismo, 1758. Bologna, folio.*
- *J. C. Wilcke's herrn Franklin's briefe von der electricitat nebst anmerkungen, 1758. Leipzig, twelves.*
- *Wilson and Hoadley's observations on a series of electrical experiments, 2d. edit. 1759, London, quarto.*
- *Æpinus's tentamen theoriæ electricitatis et magnetismi, 1759, Petersburg, quarto.*
- *J. F. Hartmann's abhandlung von der verwandschaft und æhnlichkeit der electrischen kraft mit der erscherechlichen luft erscheinungen, von 1759, Hanover, octavo.*
- *Nolle's lettres sur l'electricité, tome 1. 1749, tome 2. 1760, tome 3. 1767, Paris, twelves.*
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- *Nollet's* Leçons de physique, tome 6th. 1764, Paris, twelves.
- *J. F. Hartmann's* anmerkungen über die nothigte achtſamkeit bey erforschung der gewetter electricitat, &c. 1764. Hanover, quarto.
- *Johannes Franciscus Cigna* de novis quibusdam experimentis electricis, from the memoirs of the academy of Turin for the year 1765, quarto.
- *Lovel's* philosophical essays in three parts, 1766, London, octavo.
- *J. F. Hartmann's* electrische experimente in luftleeren raume, 1766, Hanover, twelves.
- *Amadeus Lullin's* dissertatio physica de electricitate, 1766, Geneva, octavo.

OTHER BOOKS, THE DATES OF WHICH I HAVE NOT FOUND:

*Winkler's* essai sur la nature, la cause, et les effets de l'electricité. Histoire, p. 33.  
*Beze's* poem on electricity. Histoire.  
 Observations sur l'electricité par Mr. — chirurgien de la salpetre. Hist. p. 98.  
 Nouvelle dissertation sur l'electricité par un physicien de Chartres. Histoire.  
 Mr. *Kruger's* meditations on electricity. Hall.  
 Tentamen de vi electrica ejusque phenomenis, auctore *Nic. Bammucaro*.  
*Laurentii Beraud* S. J. theoria electricitatis, Petersburg. Wilcke, p. 12.  
*Watkins* on electricity.

As the reader will see by the asterisms what books I have had an opportunity of perusing, he will see in what parts my history is most likely to be defective. And I shall think myself greatly obliged to any person, who will favour me with the use of any treatise which contains a discovery of importance. But I do not apprehend that any thing very material can have escaped me.

# I N D E X.

N. B. Whenever the second volume is meant, it is particularly expressed thus, II. When no Roman figure is prefixed to the page, the first volume is always to be understood.

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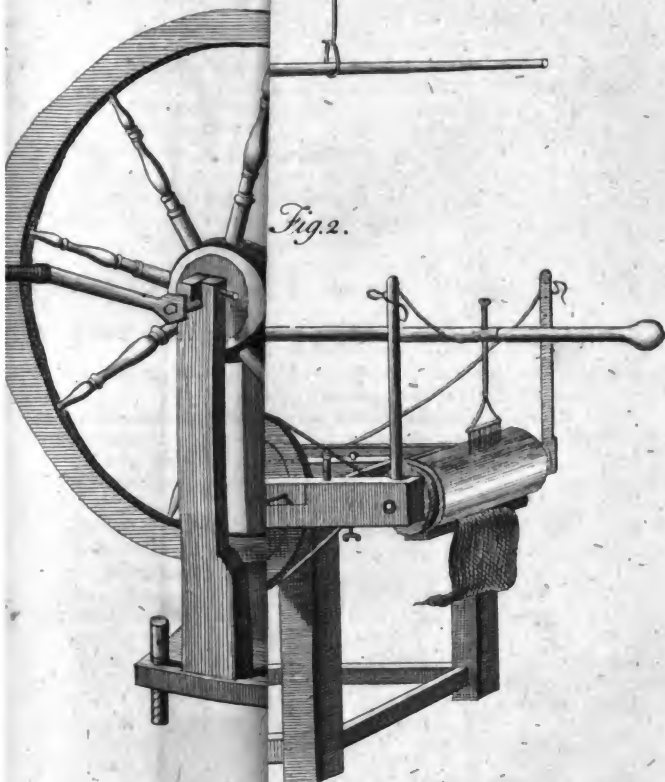
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# F I N I S.

*Hist of Electricity*

*Fig. 1.*

*Fig. 2.*



*J. H. Smith sc.*



PLATE VI.  
*His of Electricity*

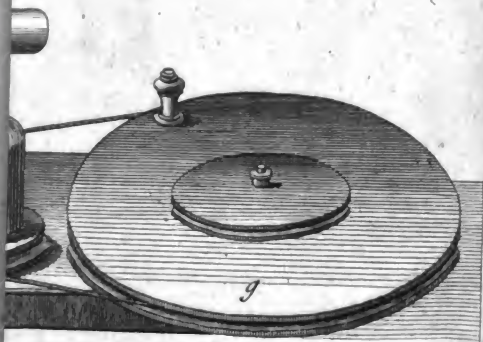
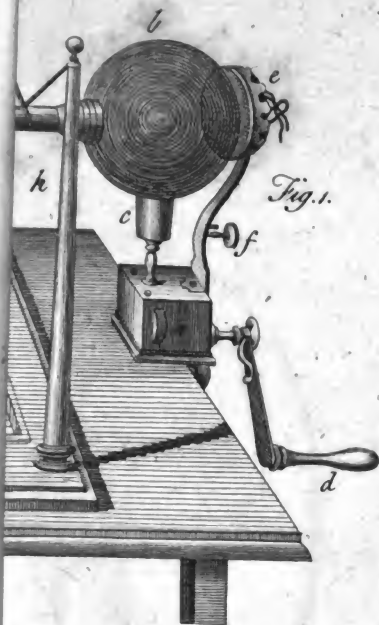
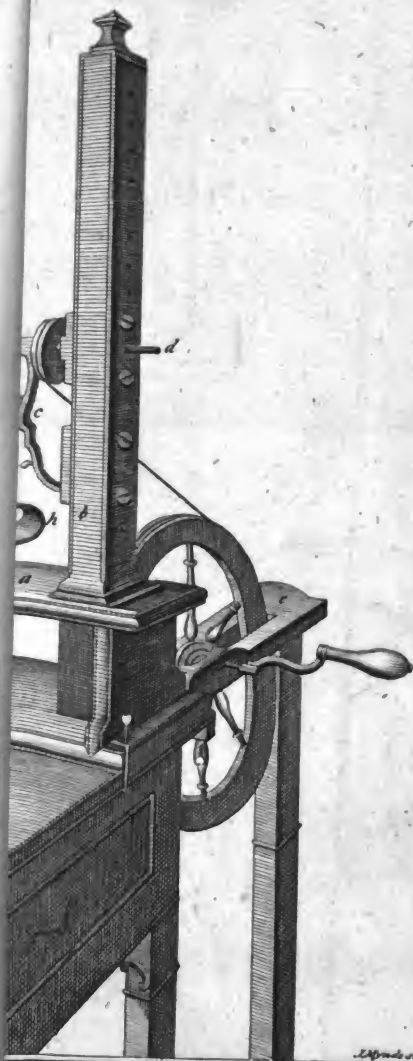




PLATE VII.  
*His of Electricity*







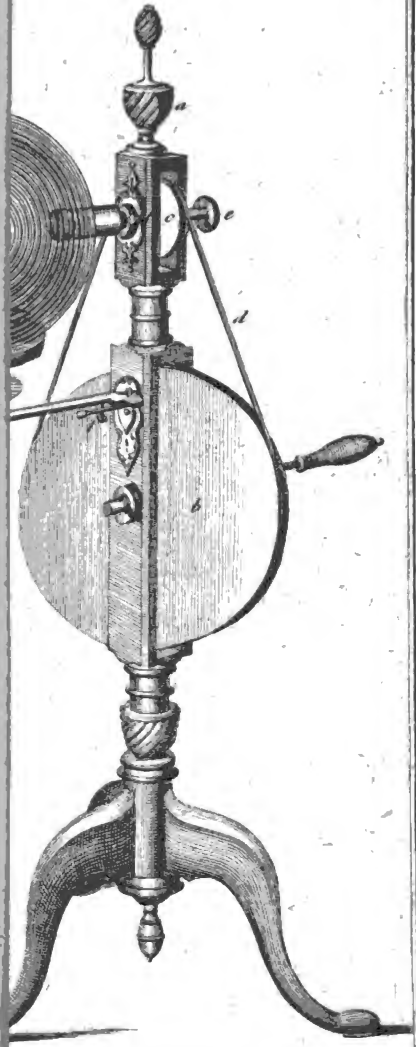




PLATE VIII.  
*His<sup>t</sup> of Electricity*

